ON HEAT

IN ITS RELATIONS TO

WATER AND STEAM:

EMBRACING NEW VIEWS

OF

VAPORIZATION, CONDENSATION, & EXPLOSIONS.

BY

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FROM THE SECOND REVISED AND EXTENDED LONDON EDITION.

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PREFACE TO THE SECOND EDITION.

A SECOND edition of this Treatise being called for by the public, I have the gratification of placing on record the following testimonial of the opinion entertained of my services, after a period of thirty-eight years devotion to the interests of the City of Dublin Steam Packet Company, to which I have referred in my preface to the first edition:

"City of Dublin Steam Packet Company Offices,
"15 Eden Quay, Dublin.

"RESOLVED, That in consideration of Mr. Williams' long and active services, the publication of the Second Edition of his Treatise on Heat in its relation to Water and Steam, shall be at the expense of the Company.

"P. HOWELL, *Secretary*."

I have here also to state, that application having been received from scientific individuals in Paris, Berlin, and Turin, for permission to translate this Treatise, and the same having been granted, editions are now in progress in the French, German, and Italian languages.

In the present edition I have placed on record the prominent part taken by the Royal Dublin Society in the advancement of the study of Chemistry in Ireland by their liberal invitation to the first Chemical Professor of the day, Humphrey Davy, in the year 1810, and their continued efforts for the promotion of Chemical Science, by a second invitation in 1811, to that distinguished Professor, and their liberal contribution for his services.

To the present edition I have been enabled to add an additional section on the subject of the JET, when brought in aid of the natural draught in the furnaces of land and marine boilers —a subject which has not hitherto received the attention due to its merits.

I have carefully revised the work, and trust that it will be received in the spirit in which it has been undertaken, namely, as an effort to partly reconcile and partly correct the anomalies which present themselves on the subject of heat in reference to liquid and vaporous bodies, and to reduce the whole to an intelligible and practical theory, based on the recognized views of Dalton, "the Nestor of the physical sciences."

Since the early sheets of this new edition of my treatise went to press, I have found that a serious misapprehension exists in the minds of some respecting the tenor of my remarks upon Dalton's views, in the chapter on the "Diffusion of Vapor." From what I have there said, it has been inferred that Dalton specifically and pointedly represented that the vapor of water formed an exception to the ordinary laws of the diffusion of gases in water; and hence I am supposed to have Dalton himself as my antagonist. This, however, is by no means the case. All that I intended to imply in the chapter referred to is, that Dalton *omits* to mention the vapor of water as one of those elastic fluids or gases which mix mechanically with water when the two are agitated together.

On this subject he says, "If a quantity of water freed from air be agitated with any kind of gas, not chemically uniting with water, it will absorb its bulk of gas." Now, because he has not applied these words to vapor (which he asserts, and all chemists admit, cannot be distinguished from permanent gases), he is not to be supposed as dissenting from the view which I have taken, viz., that the vapor of water does maintain its identity and properties as an elastic fluid, when mixed with water. The question was never raised by him, or in his time, and he has not said any thing that can be taken as contravening that view.

It may be well, then, here to add a consideration which tends to show that there are sufficient grounds for believing that the water only acts the part of a medium with which vapor mixes mechanically, as it does with other gases or elastic fluids. We know that oxygen, an elastic fluid, mixed with water retains its separate identity and the property of repulsion among its own atoms; also, that air, which is a compound of two elastic fluids, oxygen and nitrogen, when mixed with water retains its separate identity and characteristics. Nay, further, that the compound of an elastic fluid and a solid body, as carbon, forming the gas called carbonic acid, retains the separate identity of its particles when mixed with water. Why, then, should we be precluded from applying the same reasoning and analogy to the compound of oxygen and hydrogen when they form the elastic fluid vapor or steam, which, as all chemists admit, cannot be distinguished on any principle from other elastic fluids?

CHARLES WYE WILLIAMS.

LIVERPOOL, *April*, 1861.

PREFACE.

PROFESSOR MILLER opens his chapter on *heat* by observing: —"Upon the due understanding of the principles and application of heat much of the successful prosecution of chemical research depends." Under the conviction that the knowledge of these principles, as regards the application of heat to liquids, is still imperfect, and capable of a large extension, the views presented in this volume are submitted to the general reader.

The several changes of which the element, water, is susceptible, under the influence of heat, have attracted the attention of philosophers in all ages. Many have made them their special study, and have given the result of their researches in elaborate publications. So numerous, indeed, have been these works, and so universal the inquiry, that it would appear almost impossible to supply any additional facts, or throw any new light on the subject. On a close examination, however, of the works, even from the highest authority, we find so much of doubt, variance, and even contradiction, as to suggest the absolute necessity of further investigation. In pursuing the inquiry, the ablest among us will discover, not only that much has still to be learned, but more to be unlearned, before we arrive at a true knowledge of the relation which heat bears to liquids and æriform matter; and that, notwithstanding the miraculous speed with which the march of chemical science has progressed, we are still but on the threshold of this inquiry.

A consideration of the laws of heat, in reference to this department of science, is not only a branch of the highest philosophy, but is intimately connected with the ordinary concerns of com-

mercial life and social progress. Among the objects in immediate connection with this subject may be classed all that has reference to steam, and its uses in the steam-engine, whether for manufacturing purposes or in steam navigation. It involves all that belongs not only to the *generation*, but the *application* of steam, thus embracing a wide field of inquiry. In this point of view, then, the present treatise may be regarded as but an opening of the subject—an outline or programme of that inquiry which will hereafter be found to embrace meteorology, and electricity, with an ultimate attainment of perfection in the steam-engine and its wonderful adaptation to the wants and business of mankind.

It may be thought necessary, when asking for a share of public attention, that an author should account for his coming before the public on a subject which has long and largely occupied the time and labors of the highest intellect, and should state the grounds of his pretensions, when differing from those who have gone before him. To this reasonable necessity I submit, and, after the manner of Watt, have but to tell "*a plain story.*"

Not being a *professional* chemist or engineer, to differ from so many who have higher claims as public instructors, may doubtless appear presumptuous. In these days, however, no writer, whatever may be his standing, can expect that his mere *ipse dixit* should dispense with further inquiry. There are now no secrets in chemistry or philosophy. The highways of science are open to all. The laws of nature are equally accessible by all. On this we are all on a par; and the present work, like those of others, must stand or fall on its own merits. This is, in truth, as much as could be desired by any writer, no man's mere dictum being recognized as sufficient to negative the results of experimental philosophy, when examined under the inferences of right reason. Every man, according to the knowledge that is in him, stands before the great tribunal of common-sense, and under the judgment of contemporaneous information.

If, however, a very long experience, coupled with an assiduous pursuit and devotion, during above half a century, both theoretically and practically, to the subjects embraced in the following pages, can give any claim to a hearing, I yield to none in this

respect, being perhaps the oldest man living who has devoted equal time and attention to the special objects here to be considered.

In the first years of the present century, being then the acting partner in a large bleaching establishment in the north of Ireland, where the new bleaching process was being introduced, chemistry became an elementary branch of the business, and to a great extent the very source and measure of success. In this, my earliest studies were under the late William Higgins, then Professor of Chemistry to the Dublin Society, of which I was a member. I also attended the courses of the late Dr. Andrew Ure, who was then lecturing on the commercial value of the several alkalies used in the linen-bleaching process. This led to a friendship, which continued to the time of his death, with that able and accurate chemist.

Being in continued communication with him, I subsequently consulted him on many points connected with the use of coal, which was beginning to be an important feature in the progress of steam navigation; and preparatory to the publication, in 1839, of my Treatise on the Combustion of Bituminous Coal, the Doctor not only entered warmly into the subject, and in its reference to the construction of boilers, but volunteered to revise my work as it proceeded, every proof-sheet, as it came from the press, being examined and corrected by his own hand. This could not fail to give me confidence in the soundness of the views and chemical accuracy in the details which formed the basis of my treatise, and which ultimately secured its acceptance by the public, and by many of the highest authority. (See testimonials hereto appended.)

From William Higgins I heard the first enunciation of the *atomic theory*, subsequently so ably developed and enlarged by Dalton. That Higgins was the first who discovered the true elements and basis of that theory was admitted by Davy himself, and by the Swedish chemist Berzelius, and other Continental authorities. I have, indeed, the tract presented to me by Professor Higgins, in which he speaks indignantly of the denial of his claim to priority, and the jealousy of Thomson in giving Dalton the merit to which he must have known he was not entitled.

At this time the Dublin Society took a prominent part in the advancement of chemistry, and specially invited Davy to repeat those lectures, and exhibit those admirable illustrations which were then exciting so much interest at the Royal Society, London. The following extracts from the minutes of the Dublin Society explain in what manner this was brought about:

May 3rd, 1810.

Resolved, That application be made to the Royal Society, requesting that they will be pleased to dispense with the engagements of Mr. Davy, so far as to allow the Dublin Society to solicit the favor of his delivering a course of Electro-Chemical lectures, in their new Laboratory, as soon as may be convenient, after the present course of chemical lectures shall have been completed by their Professor, Mr. Higgins.

Resolved, That the sum of 400 guineas be appropriated out of the funds of the Society, to be presented to Mr. Davy, as a remuneration for the trouble and expense which they propose him to incur, and as a mark of the importance they attach to the communication which they solicit.

* * * * *

The following letter from Mr. Davy was presented, and ordered to be entered on the Minutes:

To John Leslie Foster, Esq., M. P., *Secretary to Dublin Society.*

Sir:—I had the honor of communicating your letter to the President and Council of the Royal Society, who desired me to express to you, Sir, and through you to the Dublin Society, the lively interest they feel in the prosperity of that useful public body, and the desire that they have of promoting its important objects.

On these grounds they have been pleased to permit me to be absent from the meetings of the Royal Society, during the time that may be necessary for delivering a course of lectures, at the Laboratory of the Dublin Society, in the month of November next.

Be pleased to express to the Dublin Society my grateful acknowledgments for the honor they have done me, in making such a proposition, and assure them that I shall use my best exertions to promote their views for the extension of chemical science, and every other species of useful knowledge.

I beg to be permitted to thank you, sir, for the flattering manner in which you have had the goodness to convey to me their proposal.

I am, sir, with great respect
Your obliged and obedient servant,

May 30th, 1810. H. DAVY, *Sec.* R. S.

November 8th, 1810.

Mr. Boardman presented the following Report from the Committee of Chemistry:

It appearing to the Committee that there were present this day, at Mr. Professor Davy's lecture, 337 persons, and that there were 371 admission tickets, it is the opinion of the Committee that there will not be accommodation for more than the last number.

Resolved, That the Assistant Secretary be directed not to issue any more admission tickets for Mr. Professor Davy's present course of lectures.

* * * * *

November 29th, 1810.

Resolved, That the thanks of the Society be communicated to Mr. Professor Davy, for the excellent course of lectures, which, at their request, he has delivered in their Laboratory, and to assure him that the views which led the Society to seek for these communications, have been answered even beyond their hopes; that the manner in which he has unfolded his discoveries, has not merely imparted new and valuable imformation, but further appears to have given a direction of the public mind towards Chemical and Philosophical inquiries, which cannot fail in its consequences to produce the improvement of the Sciences, Arts, and Manufactures in Ireland.

Resolved, That the thanks of the Society be communicated to the Royal Society, for their ready compliance with our request, in dispensing with the engagements of Mr. Davy, during the last six weeks.

Resolved, That Mr. Davy be requested to accept the sum of 500 guineas from the Society for his trouble.

* * * * *

June 13th, 1811.

Resolved, That a letter be written to Mr. Professor Davy, requesting him to favor the Dublin Society, and the Irish public, with a further communication of the recent discoveries in Chemical Philosophy, and to deliver a course of lectures in their Laboratory for that purpose, in the months of November and December next; and requesting that he will also repeat to them, at the same time, the course of lectures in Geological Science, which he has read this year to the Royal Institution; and that he will be so good as to procure for the Society, copies of as many of the geological sketches referred to in that course as he may think necessary for the elucidation of the subject; and further requesting him to superintend the construction of a Voltaic Battery of large plates, for the use of the Society, to be transmitted to them in time for these lectures.

Resolved, That a letter be written to the Royal Society, expressing the conviction of the Dublin Society that these communications from Mr. Davy will tend materially to the advancement of Science in Ireland, and requesting that they will be so good as to dispense with Mr. Davy's engagements at the time referred to.

Resolved, That in the event of Mr. Davy and the Royal Society acced-

ing to the wishes of the Dublin Society, transferable tickets be issued at the price of two guineas each, entitling the bearer to admission to both courses; and that the issue of these tickets, their number, and all the arrangements necessary for the accommodation of the Society and the public be referred to the direction of the Committee of Chemistry.

Resolved, That the Society feel it proper to renounce for their members any privilege of gratuitous admission to the lectures.

* * * * *

December, 1811.

[Sir Richard Steele, Bart., presented a report from the Committee of Chemistry; from which it appeared that all the different items of expense, including the geological sketches and voltaic apparatus, amounted to £327 15s. 1d.; the total amount of money received for tickets issued was £1,101 2s.; the excess of the receipt above the charge was therefore £773 6s. 11d.]

Under the circumstances your Committee beg to submit it as their unanimous opinion, and do therefore recommend, that a less sum than £750 ought not to be offered to Mr. Davy as a remuneration for the two courses.

Richard Steele in the Chair.

Resolved unanimously, That the thanks of the Society be communicated to Mr. Davy for the two excellent Courses of Lectures in Chemical and Geological Science, which, at their request, he has delivered in their laboratory, full of valuable information, and which have not merely continued, but materially increased, the spirit of philosophical research in Ireland.

Resolved unanimously, That the thanks of the Society be communicated to the Royal Society for their ready compliance with our request in dispensing with the engagements of Mr. Davy during the last six weeks.

Resolved unanimously, That Mr. Davy be requested to accept the sum of £750 as a remuneration on the part of the Society.

January 16, 1812.

The following letter from Mr. Professor Davy was read:

Dublin, Dec. 9, 1811.

Sir:—I have received your letter inclosing a draft for £750, Irish. I am very much gratified by the thanks of the Dublin Society for the Course of Lectures which I had the honor of delivering in their Laboratory, and I am proud of their opinion that they will be useful to the Irish public.

The attention, candor, and indulgence with which they were received by the audience, I shall remember with the warmest feelings of gratitude as long as I live.

I have the honor to be, Sir,
With much esteem,
Your obliged and obedient servant,
H. Davy.

B. W. Carthy, Esq.,
Sec. Dublin Society.

In the progress of his first course of lectures, in 1810, he distinctly recognized the claim of Higgins.*

On the subject of the diffusion of gases, and the necessity of providing the due equivalent of atmospheric air in effecting perfect combustion of the coal gas in the furnace, I read a paper, accompanied by illustrative diagrams, before Dalton himself, at the meeting of the British Association, in Manchester, and which met his entire approbation. Independently of a theoretic study of the chemistry of combustion as a branch of the business in which I was largely engaged in Ireland, my attention was further drawn to the relative heat-generating properties of the several kinds of fuel, including coal, and turf—called peat in England—and which excited considerable interest at the time in the north of Ireland. In our own establishment the fuel employed was turf, and, having an extensive tract of bog land presenting all the varieties of that valuable fuel, I availed myself of the abundant opportunities thus afforded of ascertaining the most efficient mode of preparing it for manufacturing purposes. Subsequently I introduced, successfully, the use of turf into steam navigation on the river Shannon, which had till then been peremptorily deemed to be impossible.

In 1806–7 I erected a large mill in Ireland, introducing a new description of machinery, by which a great additional power was applied in the process called *beetling*, and the finishing of linens. In that mill I also introduced, for the first time, the use of metal gearing, thus superseding the cumbrous use of timber for the teeth of wheels.†

* In Dublin, Davy lodged at the house of a chemist of the name of Hogan. The latter observing that he did not seem to recognize the advance that had been made on the subject, took the opportunity of asking him if he had seen Higgins' tract on the atomic theory. Finding he had not, he put it into his hand. Davy sat up late, reading it, and, on the following morning, thanking Hogan, he said, "Why, it's all here! He is right; the elements of the theory are here given."

† Having the castings made at the foundry of Edwards, in Belfast, when they were brought home my millwright absolutely refused to apply them. He had never seen any thing of the kind, and raised a strong objection to the use of iron; and it was only under the threat of dismissal that he was induced to attach them to the other part of the machinery.

As regards the study and general application of chemistry to the arts, I have since, and unremittingly, watched the progress made during the last fifty years, to which Dalton, Davy, and Faraday have so largely contributed.

In 1822, being in constant communication with the late John Oldham, then at the head of the engraving and printing department of the Bank of Ireland—one of the ablest mechanicians of the day, and who was the inventor of the system of mechanical consecutive numbering of bank notes, since adopted in the Bank of England, and subsequently, in all railway stations—our attention was directed to the improvement of the propelling properties of the outside wheels of steam vessels, in consequence of the failure of a company which had been previously established in Dublin, and for the settling of the differences between which and the contractor and builder, the late Mr. Bogy, after a protracted litigation, I was appointed arbitrator. My decision was, that one of the vessels should be handed over to the contractor, and his legal expenses paid, which were considerable.*

In connection with Mr. Oldham, and after numerous experiments on the large scale, and at the expense of above £1,000, borne by me alone, a patent was taken out for a revolving or *feathering wheel*, which went under the name of the Oldham wheel, the cost of which patent (near £400) was paid by me.

My connection with this improved mode of propulsion, and my confidence in its success, became the direct inducement for forming a company which should undertake the conveyance, not only of passengers, but of merchandize, and continue as a regular trader, throughout both winter and summer months, between Dublin and Liverpool, which had not hitherto been attempted.

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* Two vessels had been built for that company, each of which had but a single 20-horse engine, the movements of which were regulated by an ordinary fly-wheel of twenty feet diameter. In these vessels the wheels were uncovered, and being thus without paddle-boxes, the spray from them, when in motion, completely covered the vessel to such an extent that the captain resigned his command after the first trial, alleging that the rheumatism, to which he was liable, would be so increased by the continued shower of spray as to unfit him for the service.

One important peculiarity of the improved wheel was, that when applied to freight-carrying vessels, their great immersion, when laden, would not impede their free action.

This first company was then established, pursuant to the provisions of an act of the Irish Parliment, the 21st and 22d George III., called the *Anonymous Partnership Act.* The conditions of that act were peremptory. The capital was not to exceed £50,000. The partnership deed was to be registered in the name of some one of the shareholders, who was called the "*acting partner,*" he alone being subject to the bankrupt laws, all the others being "*sleeping partners,*" with a liability limited to the amount of their respective shares, but on the express condition that neither directly nor indirectly should they interfere with the management or proceedings of the *acting partner.*

Under these conditions the entire risk, responsibility, and management were thrown on that acting partner. With this condition, which I made a *sine quâ non* of my proceeding, the company was registered under the firm of Charles Wye Williams and Company, the capital of which consisted of fifty shares of £1,000 each, the partners being but thirty-two in number.

Under that firm the first two vessels, the *City of Dublin* and *Town of Liverpool,* were constructed in 1823; and it is not a little curious to look back at the difficulties that had to be encountered at that early period of steam navigation. I had then to meet the objections and allay the fears of many of the first mercantile houses, and the indirect hints from some of my own partners on my "*rashness*" in having *two* vessels built, instead of being satisfied with what was characterized as a "*safe, snug company,*" by beginning with but a *single vessel,* to feel my way. In truth, had not my partners been bound by deed not in any way to interfere, under the risk of personal liability, no more than *one vessel* would have been proceeded with in that year.

The success of the company, however, was such as to call for increased capital and more vessels. New capital was then obtained, and four additional steamers were laid down, viz., the *Hibernia, Britannia, Manchester,* and *Leeds.*

The Company having thus outgrown the provisions of the Act, took the title of the "*City of Dublin Steam Packet Company,*"

under a new deed, in which I was named the *managing* director, in whom was vested the entire executive, with a board of seven directors for the financial and other departments of the Company.

The intercourse by steam navigation continuing to increase, I applied, in 1828, to the then member for Liverpool, the late William Huskisson, and through his influence obtained the Company's Act of the 9th of George IV., entitled "An Act for regulating and enabling the City of Dublin Steam Packet Company to sue and be sued, and thereby to encourage the use of vessels propelled by steam in Ireland."

This act was obtained especially to enable the company to place suitable steam vessels on the river Shannon, where a small iron vessel, of 10 horse-power,* had already been placed by the late John Grantham, C.E., who had just finished a survey of that river under the direction of the late John Rennie, C.E.

With the view of inducing the Government to improve the navigation of that noble river, I published, in 1832, a tract on the inland navigation of Ireland generally, pointing out the capability of establishing an intercourse along that river of no less than 200 miles, from Lough Allen in the north, to the sea, forty-six miles below the City of Limerick.

The publication of that tract led to the appointment of a commission, under Colonel Burgoyne (now Lieutenant-General Sir John Fox Burgoyne), and Colonel Jones (now Lieutenant-General Sir H. D. Jones). The result of that commission was the passing of an act under which a sum of half a million sterling was expended on the improvement of the navigation.

Since then, the company have obtained further acts, namely, that of the 6th and 7th William IV., and, lastly, of the 23d Victoria. This last was obtained in the present year (1860), for the special purpose of enabling the company to raise additional capital for the construction of four steam vessels of the largest class, for the conveyance of passengers and mails between Holyhead and Kingstown within a given number of hours, under contract, and in concurrence with the London and North-Western Railway Company.

* This engine was constructed by the late Aaron Manby, at Tipton, Staffordshire, and has continued to this day in perfect working condition.

In connection with the late Francis Carleton, whom I had appointed a co-manager of the City of Dublin Steam Packet Company, we undertook the establishment of a Transatlantic Company, and began the service with the *Royal William* steamer, which vessel made several voyages to New York. For this purpose the additional steamer, the *Great Liverpool*, was purchased from the late Sir John Tobin, and this vessel also made three voyages.

This new Transatlantic Company proving to be a losing concern, was broken up under the conviction that no private association could be sufficient for such a service unless supported by an adequate public subsidy.

In conjunction with Mr. Carleton and the Directors of the then Peninsular Steam Company, a new Company was formed under the title of the Peninsular and Oriental Steam Navigation Company, the steamer the *Great Liverpool* being transferred to that Company, and forming one of the then fleet for the service of India *viâ* the Red Sea and the overland route, under Government contract.

Such was the infant state of steam navigation in 1823, when the first two vessels were constructed, that although a contract was made with the most eminent engineer of the day for the engines, and an equally eminent shipbuilder for the hull, they could not be prevailed on to confer on the many points on which a common interest and purpose might be expected to have made them equally zealous. Among these was the important question of the mode of securing the engines to the ship's timber framing. This, however, was the very point the responsibility of which both contractors were most reluctant even to consider. The engineer disclaimed any knowledge of ship-building, and would not be accountable for any thing connected with it. The shipbuilder, on the other hand, expressed his ignorance of what belonged to the steam-engines, and declined responsibility in reference to the mode or means of attaching them to the timber hull.

Feeling the vast importance of the subject, I was compelled to be my own engineer; and having had a template, or pattern, made of the bottom or bed-plate of the engines, I was obliged

with my own hand to mark the position of each of the sixteen holding-down bolts, by which the engines were to be secured to the ship's framing. As I had apprehended, not one half the number of these large bolts would have gone through floor timbers, but, passing between them, have had no other holding than that of the 4-inch outside pine planking of the vessel—manifestly wholly inadequate to the purpose of permanence or stability.

The only alternative that remained was the insertion, at a considerable expense, of additional floor timbers to fill the spaces where the holding-down bolts passed. To keep these in their places I had four additional sister kelsons added, two on each side; and on these, and through the floor timbers, the engines were successfully secured.

Strange as this will appear to the builders and engineers of the present day, nevertheless, not in the first only, but in the second vessel also, the same difficulty had to be encountered and the same process pursued. This suggested the absolute necessity of employing a competent engineer for the Company, thus to combine the operations of the contracting engineer and the shipbuilder, and which subsequently led to having but one contractor, who should undertake the construction both of the engines and the building of the hulls.

In the first vessel, in 1823, the wheels were constructed on the feathering principle of the *Oldham* wheel, already mentioned. These, however, were manufactured so imperfectly, that they were ultimately changed for the ordinary fixed floats. The principle of the wheel was, however, unobjectionable; and was subsequently adopted with some mechanical modifications, and became general under the name of *Morgan's* Feathering-wheel. Both plans may be seen in the first quarto edition of Tredgold's large work on the Steam-engine, where the preference is given to the *Oldham* wheel.

Having a deep interest in the success of steam navigation, in September, 1837, I made a communication to the mechanical section of the British Association, on Improvements in the Construction of Steam Vessels, by dividing the vessel's hull into sections, each of which should be water-tight, by the introduction of plate-iron bulkheads. It is needless to observe on the value of these

partitions; they are now universally introduced into all iron steamers, and have already been the means of preventing the loss of several vessels under collision.

The management of this Company during so many years necessarily required a continued study, with a practical application of all that belonged to the use of fuel, both on the score of efficiency and economy; in a word, to all that belonged to the generation of heat, and the application of that heat in the generation of steam. On these heads, then, I may claim as large and extensive an experience as any man living—not merely *en amateur*, but professionally and as a matter of business, both mechanically and commercially. In immediate connection with steam navigation, then, I have had the experience of thirty-seven years; while, as regards the value and use of fuel, the construction of boilers and furnaces, and all that pertains to the obtainment of perfect combustion, I may claim the devotion of a lifetime.

Of this latter, the result of my long experience has been given in the treatise on Combustion, under the immediate correction of the late Dr. Ure, already referred to—a work now in its fourth edition, it having also been translated into French under the immediate auspices of the Minister of Marine in France.

Of the former, as regards the generation and application of steam, the present treatise may be described as a selection of extracts from the laboratory memoranda of the last fifteen or twenty years.

On the last subject it has in general been taken for granted that the theories of those who have already embodied their views in numerous publications were unassailable, and had exhausted the subject. In common with others, I participated in that confidence. Embarrassed, however, by the anomalies and contradictions which continually presented themselves, there appeared no alternative but that of undergoing the labor of experimenting and examining for myself. In pursuing this during a series of years, it became evident that much of the complication in which the subject was involved was purely imaginary, and was the result of beginning at the wrong end of the inquiry, and assuming bases for which there was no reliable authority. With all

my devotion to Watt in particular, in which I yield to no man, there was no alternative, under the force of conviction and the "inexorable logic of facts," but the admission that there was some source of error both in his theory and practice which had still to be proved, in reference even to his great discovery of the condensation of steam in his engine.*

Convinced of the value of experimental philosophy over mere theory, instead of looking for truth by the aid of mathematical formulæ, which but too often perpetuated and gave a *quasi* consistence to error, I followed the systems adopted by Davy, and mainly by Dalton, of reducing each view to a visible representation—thus bringing physical certainty in aid of theory, adhering to Dalton's conviction that *"no conception was clearly grasped by the intellect if it could not be visibly embodied to the outward senses"*—(a dictum which is here strongly recommended to the attention of all theorists and experimenters), thus studying nature's laws under the process of experiment, which, as Roger Bacon observes, is at the root of all our senses, and further, that "among the grounds of human ignorance are, the trusting to inadequate authority and the force of custom. As the slaves of habit, we are still found following the untaught crowd, and flinch from the wholesome phrase, *we do not know*, the remedy for which is honest research, original and independent thought, with strict truth in the comparison of what is already known by others."

In the course of my experiments, I became convinced of the *unity* and *simplicity* of the mode by which those laws of nature are regulated whereby the wondrous and effective power of steam is brought about. Under those laws, each single atom of a liquid, by the influence of what we call heat or electricity, becomes endowed with peculiar properties, while mere accumula-

* "Of all expounders of a great discovery, it is well known that the discoverer himself is one of the worst. Nature, in truth, divides her work. To one man she assigns the task of originating a new thought: to another, that of imparting to it a fitting shape, and adapting it to the uses of mankind. So discoveries become known and spread. The popularizer succeeds to the philosopher. Sometimes these co-workers only succeed each other at long intervals. Sometimes the expositor follows quick upon the thinker: but, quickly or slowly, he must come. The *how*, is no less essential than the *what*."—*Physiological Riddles.*

tion, or increase in number, becomes sufficient to produce all we experience in the changes of temperature, expansion of volume, and pressure.

No claim is here made to original thought, such as we rightly attribute to Watt in his conception of the separate vessel—the condenser. Merit is claimed alone for a patient and persevering inquiry into the processes of nature. Pursuing a similar course, our study should be not in the vainglorious and pedantic display of mathematical formulæ, in the manner of Pambour and some others, but in an examination of the materially-useful laws which govern the union of heat with liquid matter, and the development of physical causes by physical means—thus tracing the simplest processes of nature to the production of the most complex results.

Hitherto, in merely assuming the existence of certain laws, we have been too prone to reject whatever appeared to be opposed to them or to recognized authority. Disbelief then became the resource of uninquiring minds, not unfrequently accompanied by a fling of ridicule, or a charge of presumption, against such as differ from those who have gone before them, as if every advance in knowledge was not equally open to the same charge. The views stated in the following pages may possibly have to undergo the same process of disbelief. We, however, no longer live in a Galilean age. Public opinion and judgment can no longer be controlled by an uninquiring submission to supposed authority, or dictated to by the unreasoning censure of custom. *Magna est veritas et prævalebit.*

Finally, in the following pages I have shown that there are sufficient scientific and reasonable grounds for asserting—

1st. That water, or its atoms, can neither be *heated* nor *expanded*, and still retain the character of liquidity, and the property of attraction among their fellow atoms.

2d. That the prevailing theories as regards *ebullition*, are altogether erroneous.

3d. That the so-called *boiling-point*, as regards temperature, is merely that point at which the water is charged with vapor *to saturation*, under the true Daltonian theory, the water acting the part of a mere *vacuum*.

4th. That we have strong grounds for believing that there is no difference between the *cause* which produces divergence and mutual repulsion among the atoms of a liquid on becoming vapor, and that which produces a similar divergence and repulsion in the pith-balls or gold leaves of the electroscope.

5th. That if there be such a thing as *Thermo-Electricity*, we are warranted in concluding that it acts, in the same way, and on a similar principle, on atoms of a liquid as on those of other bodies.

6th. That we have rational grounds for believing that *explosions* in steam-boilers are frequently the result of the accumulated steam (present in the body of the water) being suddenly released by the removal of the pressure from the *denser* medium of the water into the *lighter* one of the air.

7th. That Watt's theory of steam being *condensed*, and reconverted into the liquid state, by the direct action of cold water, is altogether erroneous.

8th. That vapor or steam cannot give out its heat to water, and is but mixed, *mechanically*, with it, on the true Daltonian theory.

9th. That we have no grounds for inferring that there can be more *units of heat* in a body of steam than there are distinct *atoms* of the matter of water, each of such atoms having its own equivalent of heat, electricity or otherwise combined with it—accumulation being the sole grounds of expansion and pressure.

CHARLES WYE WILLIAMS.

LIST OF AUTHORITIES QUOTED OR REFERRED TO IN THE FOLLOWING PAGES.

Dalton.
Watt.
Dr. Black.
Dr. Thomson.
Dr. Henry.
Professor Faraday.
Professor Brand.
Professor Graham.
Professor Miller.
Professor Rankine.
Sir Robert Kane.
Dr. Ure.
Dr. Reid.
Sir John Herschel.
Dr. Turner.
Dr. Lardner.
John Scott Russell.
Professor Dixon.
Comte de Pambour.
M. Regnault.
Professor Gmelin.
M. Arago.
M. Denny.
M. Magnus.
Encyclopædia Britannica.
English Cyclopædia.

SECTION I.

OF THE THREE STATES OF WATER.

In this branch of the inquiry we have to consider the difference, chemically, physically, and dynamically, between the several states in which the element, water, has to be dealt with, and the identities which characterize each; these are:

1st.	Crystallized,	as Ice.
2d.	Liquid,	as Water.
3d.	Gaseous, or Æriform,	as Vapor.

The conditions and properties of water in these several states are determined solely by the equivalents or quantities of heat combined with the water in each atom respectively.

Of the chemical constituents of water, each atom is a compound of one equivalent of hydrogen and one of oxygen. Upon the relative weights and volumes of these all chemical authorities are agreed, and they need not here be further considered. The properties of water, physically considered, when in the state of *ice*, resemble those of other solid bodies in this respect, that its atoms are in close contact, have strong cohesive powers, and are incapable of motion or change of position, *inter se.*

In the state of *liquid*, immobility is changed to mobility, with a strong *attraction* among its several atoms or particles.

In the state of *vapor*, further changes take place. *Attraction* and *mobility* give way to mutual *repulsion* and *divergence.*

We are sensible of the presence of water atoms in almost all states and forms of matter, and of the difficulty with which chemists have to contend from their presence.

The quantities of heat inherent in water in each of its three states are, in the general opinion of chemists, as follows, viz.: the *latent heat* of ice 40°, that of liquid, 140°, and that of vapor, 1,000°. The first two are supposed to be ascertained by certain physical tests; the last, however, can only be received as an approximation to what cannot be determined with any certainty.

If, then, the maximum heat contained in ice be 40° latent and 32° sensible, the inference would be that each atom of the crystallized mass, on receiving an additional unit of heat, would have its statical conditions altered; that, losing its crystallized form, it would separate from the mass, and become part of a fluid or liquid body.

The same process, necessarily, would take place on its receiving a further unit of heat beyond what it is capable of retaining in the liquid state. Its *status* would then undergo a further change, and become gaseous or vapor. In both cases, namely, the passing from the solid to the liquid, and from that to the state of vapor, we see the remarkable changes which supervene, distinguishing the peculiar characteristics of each.

When Dalton first enunciated the property in elastic fluids of *mutual repulsion*, the immediate effect of which he termed *diffusion*, he also showed that "vaporized bodies cannot be distinguished, on any scientific principles, from *permanent* elastic fluids." These results should be borne in mind throughout the inquiry, as they form the bases of those effects which are exhibited in the various combinations of heat with liquids of all descriptions, from water to mercury. This principle of *mutual repulsion*, whatever

may be the cause, becomes then the main element, which, in opposition to gravity, forms the great characteristic of all *elastic fluids*.

We will now endeavor, symbolically, to mark the several combinations of heat with the matter of water in its three several states. On heat being applied to a body of ice, the cohesive property of the constituent particles, which come into contact with it, is lost. The particles separate from each other, and, falling from the mass by the force of gravity alone, form a liquid.

Now, as the thermometric temperature of the ice was 32°, and that of the liquid also 32°, it follows that the entire of the heat communicated, and by which the change was effected, must be considered as in the *latent* state. If an atom of ice, then, be represented by $HO+C^c$—namely, one equivalent of hydrogen, one of oxygen, and one of caloric, or heat—the liquid atom will consequently be $HO+C^{c2}$, or HO, plus two of caloric.

If heat be continued, such of the liquid atoms as may receive each an additional unit will then assume the vaporous form, and, as it had previously received its full complement of *latent* heat, this additional unit will consequently be *sensible*, or available heat, and as such will act on the thermometer. The atom of vapor may then be symbolized thus:

HOC^{c3}, or, H, Hydrogen.
O, Oxygen.
$C^c\,C^c\,C^c$, Caloric.

Vapor atoms may then be stated as the union of one atom of the liquid and three units of the heat—say two of latent, and one of sensible.

We will now proceed to consider practically the generation of vapor, from water, in the mass.

3

SECTION II.

VAPORIZATION.

There is nothing more remarkable than that with so many works proceeding from writers of high authority, and on a subject that has so deeply engaged the attention of philosophers and chemists, there should still be so much that is but imperfectly understood respecting heat, in connection with water. No writer has yet given satisfactory views on the following points, viz.:

1st. What is vapor?

2d. How and where is it formed?

3d. What are its special properties?

4th. In what does it differ physically and dynamically from water?

5th. What are the relative proportions of latent and sensible heat in either?

6th. What relation has it to electricity?

All assume that the statements of those who profess to have examined the subject were based on well-defined and accurate experiments. The various modes, however, in which the process, and even the term *vaporization*, have been described, have in no small degree complicated the subject, and manifestly suggest the necessity for further inquiry. Had writers concurred in adopting any one theory of vaporization, there would have been less room for misgivings. When, however, we find scarcely any two agreeing, even on elementary points, this alone is sufficient to raise serious doubts as to whether any have given a true account of the process.

Strictly speaking, *vaporization* means the single process of converting atoms of a liquid into those of vapor. Numerous instances might here be given, not only of the misapplication, both of the term and the process, but of the confounding it with others; in particular, with that of *evaporation*. Turner* says: "Vaporization is conveniently studied under two heads—*ebullition* and *evaporation*. In the first, the production of vapor is so rapid, that its escape gives rise to visible commotion in the liquid. In the second, it passes off quietly."

That vaporization cannot be studied under either head is evident, seeing that vapor may be formed without ebullition or any visible commotion whatever; and as to rapidity—that being solely determined by the rate at which heat is absorbed by the liquid—as much vapor will be generated, in any given time, by the same quantity of heat, whether with or without ebullition. In contradiction to this statement, then, it may, *in limine*, be broadly said, that neither ebullition nor evaporation have any immediate connection with vaporization.

Dr. Lardner gives a different view of the subject, viz.: "When a liquid boils, vapor is formed in *every part* of its dimensions, and more particularly in those parts which are nearest the source of heat; but liquids generate vapor from *their surfaces* at all temperatures." How vapor can be generated at *the surface* of a liquid without a further application of heat, is an unexplained mystery. Equally so when it is said, "Vapor is formed in *every part* of its dimensions." In such case, where is the heat to come from by which the liquid atoms are converted into vapor, or how is it to arrive at the interior of a body of water?

* "Elements of Chemistry," by Edward Turner, edited by Baron Liebig, Professor of Chemistry in the University of Giessen.

In a popular work on steam, we have an epitome of the almost universally received theory, and which it is here proposed to question and mainly to disprove:

"When heat is first applied to a body of water, a rapid circulation of the fluid ensues. The water at the bottom being first heated and expanded, becomes lighter than the rest, rises to the top, and is replaced by the current of cooler water descending to receive in its turn a further accession. By-and-by small globules of *steam*, formed at the bottom, and surrounded by a film of water, are observed adhering to the glass: as the heat increases they enlarge; in a short time several of them unite, form a bubble larger than the others, and detaching themselves from the glass, rise upwards in the fluid. But they never reach the surface; they encounter the currents of water still comparatively cold, and, descending to receive from the bottom their supply of heat, *shrivel up* into their original bulk, and are lost among the other particles of water. In a short time the mass of the water becomes uniformly heated; the bubbles, becoming larger and more frequent, are *condensed* with a loud crackling noise; and at last, when the heat of the whole mass reaches 212°, the bubbles from the bottom rise *without condensation* through the water, swell and unite with others as they rise, and burst out upon the air in a copious volume of steam, *of the same heat as the water* from which they are formed, and pushing aside the air, make room for themselves."*

A statement so precise in details, and sanctioned by so high an authority in all matters connected with the steam-engine, would appear beyond question. Many proofs, how-

* "The Steam-Engine," by John Scott Russell, M.A., F.R.S.E. The authorship of the paragraph quoted is attributed by Mr. Russell to Sir J. Robinson.

ever, of results directly at issue with the above have suggested doubts, in which that authority would assuredly have concurred had time and opportunity been afforded for examining them.

The following remarks may, however, here be made in reference to some of the points in this statement, viz.:

1st. The heating and expanding of the liquid are both here assumed without proof or even inquiry.

2d. The water "becoming lighter, rises to the top, and is replaced by the colder water." No sufficient reason, however, is given for this replacement. An ascending lighter body would necessarily remain at the top, as a cork would.

3d. Globules of steam never adhere to any thing; they have no such power or property. It is only when reconverted into the liquid state that adhesion becomes available.

4th. Globules, either of water or air, remain always visible up to the surface.

5th. The idea of bubbles of steam being *condensed* in their ascent, is wholly inadmissible, and contrary to fact.

6th. The loud crackling noise is here assumed to occur in the body of the water; this also is contrary to the fact.

7th. As to "the steam being of the same heat as the water from which they are formed," that is simply impossible, unless by ignoring the effect of heat.

The following analysis will enable us to detect some of the oversights in the above statement, and give a more accurate view of the process.

Water undistilled and unfiltered being put into a glass beaker, over an Argand burner, numerous small globules will shortly be seen adhering to the bottom and sides of the glass. These have been mistaken by many writers for

new-formed vapor, and, as above stated, for *globules of steam.* They are, however, mere globules *of air*, invisible at first by reason of their minuteness, but becoming enlarged as the glass to which they adhere becomes heated, and further, increasing by accumulation, they become visible, often enlarging to the size of 1-10th of an inch, and adhering to the glass with such tenacity (if the process be carried on gently) that they may be even touched with a fine wire, and swayed from side to side before they are dislodged. These not unfrequently remain adhering to the bottom until ebullition has begun to agitate the mass. When they are detached, they rise, not with a zigzag motion, as described by many, but in a spiral manner, as they ascend.

That these globules have no relation to vapor is proved by the fact, that if, by being previously boiled and filtered, the water has been deprived of its air (of which it contains about two per cent.), and if on being cooled, the process be immediately repeated, no such globules will appear.

It has already been stated that on atoms of a liquid becoming atoms of vapor, by the addition of heat, their characteristics are entirely altered;—mutual attraction and mobility being changed to mutual repulsion and separation, with an increase of volume to an extent which makes them lighter than the surrounding atoms of the liquid. On these newly-acquired properties depend the whole phenomena which steam exhibits. "Steam," according to Dalton, being "an elastic fluid like common air, and possessed of similar mechanical properties."

On this Sir Robert Kane observes, "The particles of volatile bodies repel each other at all temperatures, *until they occupy completely the space in which the body is contained*, and exercise a pressure which is equal to the force

of their mutual repulsion, and which is termed the *elasticity of vapor.*" We here recognize the elements of divergence or diffusion, force and pressure in volatile bodies.

An important question then arises, namely, whether atoms of vapor, on their formation, retain and exercise their several properties *as an elastic fluid,* while they remain *in the body of the water* in which they have been generated, and before their escape into the air. As this will hereafter be the *quæstio vexata,* a rigid inquiry into the process of the union of heat with liquids becomes necessary.

As the change from the liquid to the vaporous state is the direct result of the union of the water with further increments of heat, it is a matter of indifference from what quarter or direction that heat may be derived, whether from *above,* as from the rays of the sun, or temperature of the air; or from *beneath,* as when heat is artificially applied.

First, with reference to the heat applied from *above.* In this case, the upper or surface stratum of liquid atoms must necessarily be *in absolute contact with the air which rests upon it.* On the heat radiating downwards upon the atoms forming this *surface-stratum,* each will absorb one or more units of such heat, converting it into the form and state of vapor, with its properties of increased volume and levity.

Here, then, is a clear, intelligible case of *vaporization.* These atoms of liquid being converted into atoms of vapor, and being subject alone to the incumbent weight of the atmosphere, there is nothing to prevent the full development of their volume. The enlarged volume, arising from the difference between the states of liquid and vapor, has been estimated as the difference between a cubic inch and a cubic foot; or the bulk is increased 1,728 times. How far the experiments are trustworthy on which this enlarged

volume has been estimated, seems seriously open to doubt. This, however, is not the place for such an inquiry.

The vaporization of this surface-stratum being effected, its atoms will rise into the air, and be replaced by those next in succession, until the whole has passed away in vapor. In this way, lakes and pools of water are vaporized; the ground becomes dried, and the atmosphere replenished with vapor, which in its turn descends in the form of rain or dew.

We have next to examine the process when heat is artificially applied *to the bottom* of a vessel containing water.

The liquid atoms forming the lowest stratum may be compared to a carpet spread on the bottom, and nearest the source of heat. The process here, as regards the absorption of heat and the change in the form and character of the liquid atoms, is necessarily the same as when applied to the upper stratum; in this case, however, each atom of the latter received its heat *direct* from the source *above it*, whereas those of the former received it, *by conduction*, through the vessel in which the water was contained.

But now comes the condition to which special attention is required, as it involves the main features of the theory here contended for. The *surface-stratum* of liquid atoms on being vaporized rose into the air as a bird from the ground, or as a balloon, on obtaining the required levity—impeded alone by the incumbent pressure of the atmosphere. Not so those of the *lowest or carpet-stratum.* These have a new element and a new obstruction to contend with. They find themselves not in contact with the *light medium of the air*, and a pressure of but 15 pounds to the square inch, but in a *medium of water*, which has a density

830 times greater than that of the air. The necessary result is that an atom of vapor, generated at the bottom of a mass of water, has to force its way upwards through this dense medium before it can reach the surface and come into contact with the air.

While, then, the surface-atoms on becoming vapor were enabled to expand, say 1,728 times that of their liquid volumes, under *atmospheric pressure*, it is manifest that those formed at *the bottom* must be influenced by the *additional density and pressure of the liquid medium* in which they were generated, and through which they had to work their way.

Again, not only had they to ascend, but to diverge and diffuse themselves by virtue of their mutually repellent principle. So numerous, however, are these vapor-atoms, and so rapid is their generation, that a portion of them are found reaching the surface and escaping into the air, almost instantaneously after the heat has been applied. Abundant proofs of this will hereafter be given.

Let us now illustrate the double process of atoms vaporized at the *surface*, and at *the bottom* of a body of water. Let *a a*, Fig. 1, represent the surface-stratum of the water in a glass beaker. Here each atom is necessarily *in absolute contact with the air above it*, and held in its position by the mere force of gravity and attraction to its fellow atoms. Let *b* represent one of these atoms, after having received its complement of heat, and, becoming vapor, rising in the air as a balloon from the ground. Its volume in this case will continue to be enlarged as it ascends, and as the balloon would, in the ratio of the diminishing density and pressure of the atmosphere in the upper regions.*

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*The circle *b* here represents not the atom itself, but the range of the repellent influence it exerts.

In Fig. 2 let *a′ a′* represent the carpet-stratum of liquid atoms, and *b′* one of them, on becoming vapor rising to the surface as a cork would—its volume being enlarged under the influence and in the inverse ratio of the density and pressure of the liquid medium in which it was generated and through which it had to pass. It will now be seen that it could only be on reaching the surface that it would be in a position corresponding with *b*, in Fig. 1, and be enabled to develop its full volume.

Fig. 1.

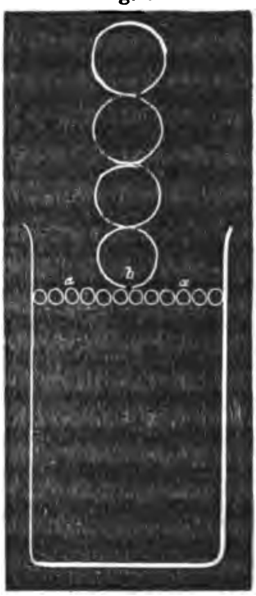
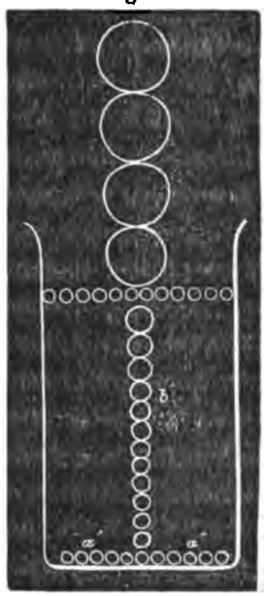


Fig. 2.



The ratio of the enlargement of the atom *b′* Fig. 2, *while in the water*, may approximately be estimated by reference to the gross enlargement of the mass. Dr. Ure estimates this at 1-25th of its liquid volume. Dalton's calculation gives a less increase.

Having considered the process as regards individual

atoms, let us now examine it in reference to the several succeeding strata, rising from the bottom as they successively become vaporized. These *carpet-strata* cannot rise in unbroken masses, as a carpet would from the floor, seeing that no atom can rise, *mero motu*, but only as contiguous and heavier atoms take their place, and *force them upwards*, as Dr. Ure expresses. The necessary result is, that each of these lower strata breaks into sections, and rises into detached portions and forms. This is fully corroborated by their appearance as seen rising through the water, if looked at across the light, or when a lighted taper is observed through the mass. At first wave-like forms will be perceived moving across the bottom, and then rising through the body of the water with a cloud-like appearance, as in Fig. 3. These will continue visible until the agitation, caused by ebullition, disturbs the uniformity of their motion. They may, however, be distinguished, even after 212° has been reached, if the process be carried on gradually and without ebullition, or internal commotion, as will hereafter be described.

It is here worthy of note, and in proof of the necessity of further inquiry, that these movements, although so palpable and suggestive, do not appear to have been recorded, or even noticed by any of the numerous experimenters from Black or Leslie down to the present time. Several other important but hitherto unnoticed movements will hereafter be pointed out.

The first tangible proof we have of the formation and absolute existence of vapor will be its appearance above the surface. The new-formed vapor having passed upwards through the liquid mass, whatever may be its depth, may be caught and condensed on a mirror or dial glass. (A little cold water placed in the dial will prevent its becoming heated, and thus favor the condensation.)

While the vapor continues to rise and escape into the air, we also find the temperature in the mass increasing so uniformly as to justify the inference of its diffusion under the Daltonian law.

Attempts were made, by the aid of an experienced optician, to have those cloudy vaporous forms magnified and projected against a large surface by the magic lanthorn; they were, however, found to be so transparent that the object was defeated. Their visibility was a mere optical effect arising from the different densities of the vaporous cloud and that of the colder water, the rays of the light being refracted from the convex surfaces of liquid particles forced upwards by the vapor, which latter would, of itself, have been invisible.

The following may be adduced in proof of the rapid formation of the vapor and its dynamic effect, after having passed through the body of water in which it was generated. Let a pound weight of water be put in a beaker, as in Fig. 3, with an Argand burner under it. On the beaker place a glass cup, *b*, also having some cold water in it. On heat being applied, the cloud-like vapor will have scarcely reached the surface when it will be seen condensed on the under side of the cup *b*, and, after a few minutes, falling in drops into the water beneath it. The experiment may be still more effective if a second glass cup, *c*, be placed on the first, the vabor formed in the cup *b* being condensed on cup *c*, after having passed through the water.

Fig. 3.

We have here then visible and physical proof,

1st. Of the rapidity with which vapor is formed.

2d. Of its diffusion, and the consequent homogeneous temperature throughout.

3d. Of the identity in a dynamic point of view as *an elastic fluid* of the vapor thus formed and escaping, after having passed through the liquid mass in each vessel.

The following experiment will further prove that it is *vapor*, and not *heated water*, that is seen rising through the water.

In the annexed Fig. 4 let *a* represent an inverted glass tundish attached to a rod, by which it may be kept down in the water in a beaker; the level of which is at *c* in the tundish, and *d* in the beaker. On heat being applied from beneath, vapor will be formed, and rising, will displace the air and water, and, pressing down on the surface, will escape from under the tundish in large intermitting masses. To make the experiment still more con-

Fig. 4.

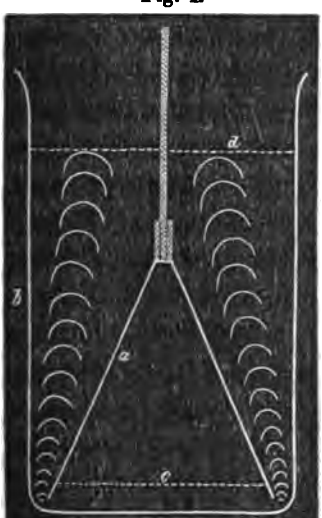
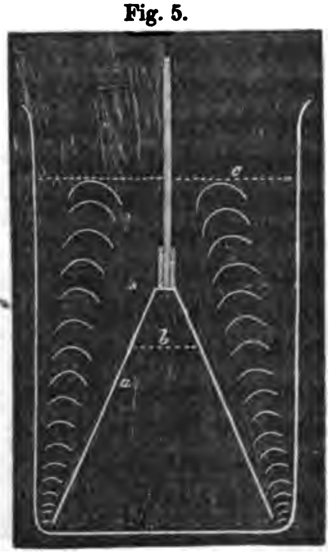


Fig. 5.



vincing, let the tundish *a*, Fig. 5, be filled with water by first holding it on its side. The level of the water in the tundish will then be at *b*, and in the beaker at *c*. On the vapor being formed at the bottom it will rise *through the water*, and, collecting in the upper part of the tundish, displace the water, and force it from under the edge of the glass, when the process of alternate filling and emptying will proceed as before.

Among the proofs of the rapid generation of vapor on the smallest addition of heat is the following. In a small 3 or 4 ounce beaker pour a little water, just enough to cover the bottom, leaving the body of the glass free for the reception of the vapor. Place the glass on the palm of the hand, or on a warm flannel and on it lay a mirror, or glass saucer, with a little cold water in it, the mere heat of the hand or warm flannel being sufficient for the conversion of liquid into vapor atoms. The vapor will soon appear condensed on the saucer. We even see the liquid particles of the ink with which we write, vaporized by the heat of the paper and that of the surrounding air.

To show the quantity of vapor formed in a short time and its diffusion through a large space, take a beaker, Fig. 6, capable of holding six or eight pounds of water. Pour in as much water, of the temperature of the surrounding air, as will cover the bottom, as at *b*. On the beaker lay a glass saucer *c*, with a little cold water in it. Lay it on some flannel, previously heated, *a*. Almost instantaneously the vapor generated by the heat from the flannel will rise, fill the glass, and be condensed on the saucer *c*. We here find that the beaker must have been full of vapor before it could reach the saucer. From this we learn, 1st, the small quantity of heat required to convert liquid atoms into vapor. 2nd. That the vapor becoming *dif-*

fused fills the entire space, as if it were a vacuum. 3d. The rapidity of the reconversion into liquid atoms.

Fig. 6.

Having examined the process of generating vapor, we next proceed to consider the peculiar properties with which it becomes endowed.

SECTION III.

ON THE DIFFUSION OF VAPOR AND OTHER ELASTIC FLUIDS.

BEFORE proceeding with the details of further processes in connection with the communication of heat to water, it is necessary that we should have a correct view of the leading principle which influences those processes. The vapor of water is admitted by all to be an *elastic fluid.* With this we associate *pressure*, or *force.* The term *elasticity*, however, has no legitimate reference to such properties in connection with liquids or vapors *en masse*, or as a single body. Elasticity is rightly defined as "the force in bodies by which they endeavor to restore themselves to their previous position or form." This is intelligible when speaking of a spring, or a sponge. But vapor and air are not such bodies, nor can they be spoken of as distinct substances. Both are mere aggregates of separate bodies, each of which, we have seen, is endowed with a property of repulsion among the atoms of its own kind, and which thus becomes the element of what is called the *elasticity* of the body. To this, then, attention should be directed, that we may have in our minds a distinct meaning of the term in reference to liquid, gaseous, or æriform masses.

On this subject Professor Faraday observes: "We have but very imperfect notions of the real and intimate conditions of the particles of a body existing in the solid, the liquid, or gaseous states; but, when we speak of the gaseous state as being due to the *mutual repulsion of the particles*, or of their atmospheres, although we may err in imagining each particle to be a little nucleus to an atmosphere of heat,

or electricity, or any other agent, we are still not likely to be in error in considering the elasticity as dependent on *mutuality of action.*" This may be said to bear directly on the question of *unity* as the representative of heat or vapor, and the *mutuality of action* in such units when they form an aggregate or mass.

Professor Rankine rightly observes: "A perfect gas is a substance in such a condition that the total pressure exerted by any number of portions of it at a given temperature, against the sides of a vessel in which they are enclosed, is *the sum of the pressure which each such portion would exert* if enclosed in the vessel separately, at the same temperature. In other words, a substance in which the tendency to expand of *each appreciable mass, how small soever*, that is diffused through a given space, is a property independent of the pressure of other masses within the same space."

This is but a more elaborate exposition of Dalton's theory of mutual repulsion, and consequent diffusion. In reference to fluids of any kind, "*each such portion*" must then have reference to *each separate atom* or particle. It would thus be the same thing had he said that the pressure exerted by any gas or elastic fluid is the sum of the repulsive forces which the several atoms exercise when *en masse.* As each atom of vapor, then, represents *a unit* of heat and repulsive force, so the amount of heat or pressure must be the sum of the units exercising such force. Temperature and pressure are, therefore, but co-efficients of the *quantity and number of atoms* present in any given space.

The Professor then draws this legitimate inference—that "Divergence or expansion is a property independent of the pressure of *other masses within the same space.*" This, again, is Dalton's law, that each gas or elastic fluid *enters as into a vacuum.* Now the whole practical effect

of vapor is involved in this law. The important elementary questions, then, are:

1st. Is vapor an elastic fluid?

2d. Has it the properties of other elastic fluids?

3d. Does it exert those properties, "independent of other masses, in bodies in the same space?".

The correctness and extent of the analogy between vapor and other elastic fluids must then be first ascertained.

"The *density of the air*," according to the latest authority, "is the result of the pressure to which it is subject. The air is an elastic fluid, that is, its bulk increases and its density diminishes whenever the external pressure, is wholly or partially removed." (English Cyclopædia, *Air*.)

Again, "This *repulsive force* of the particles of air, of which we know nothing but its effects, is a counterbalancing force *from within*, to the pressure *from without*, and is greater or less, according to the greater or less *nearness of the particles*. In other words, the elastic force of the atmosphere as distinguished from the superincumbent column of air." *So of the vapor in water*. As an elastic fluid, there is the *repulsive force* of its several atoms acting as a counterbalancing force *from within* to the pressure *from without*. Let us then keep in mind this repulsive force, or mutually repellent action, as being the direct source of what is called the pressure of the mass. Vapor, then, but follows the law of other elastic fluids, when relieved from the pressure *from without*, that is, from the *surrounding medium*, whatever it may be, "its bulk increasing and its density diminishing, as the external pressure is wholly or partially removed." This, it will be seen, is precisely the case when the vapor particles, on their escape into the lighter medium of the air, are removed from the pressure of the denser medium of the water.

The same authority adds, "as we ascend in the atmosphere the superincumbent column of air becomes of less weight, and the density becomes less; that is, a cubic foot at the height of 1,000 feet above the ground is not so heavy, nor *does it contain so much air*, as a cubic foot at the surface of the earth." *So of the vapor*, as it rises from the bottom of the vessel to the surface, and thence into the air. Now as the elastic medium, the air, presses equally on all sides and in all directions, why shall not the same be said of the denser medium of the water acting on the several particles of the vapor in it? The question of bulk, then, is a mere question of quantity or number in given spaces, each particle, however, still preserving its identity from the moment of its generation until it reaches the highest region in the air.

Again—"The air, having in itself a force which tends to *separate its particles* from one another, or to *expand the whole bulk*, but which grows less and less as the particles are more and more separated, that is, as the bulk increases." Now, what is *this force*, in itself, which so tends to separate its particles from one another, but the *mutually repellent property* inherent in the constitution of the vapor atoms? This has already been referred to *thermo-electricity*, as, with equal propriety, we speak of the force which separates the leaves of the electrometer, and seeing that we know of no power in nature capable of producing this tendency of particles *to separate or repel each other* but electricity, we have then but to substitute the elastic fluid *vapor*, for that of *air*, and the description is at once analogous and accurate.

When we speak of a body of water, filled or saturated with vapor, we may equally describe it as we do of air, namely, as being that state in which "the elastic force on

a square inch of the surface of the air arising *from its own constitution*, just balances the pressure upon that square inch." In other words, as the state of equilibrium which just balances the pressure of the medium in which it exists, whether that be water or air. This is further inferred when it is said that, from "careful experiments, it appears that air and all other gases, as well as vapors, and also all mixtures of gases and vapors, obtain an increase of elastic force for every increase of temperature, and expand, therefore, if expansion be possible in the vessel which contains them. Thus, dilatation is uniform; that is, whatever expansion arises from an increase of 12° of temperature, half as much arises from an increase of 6°; twice as much from one of 24°, and so on. This remarkable law was discovered nearly at the same time by Dalton in England, and Gay Lussac, in France."

Throughout we find, that *increase of temperature* is regarded as the basis, or cause of the several changes of pressure, expansion, or elastic force. The imparting of heat is one thing, but the indicated temperature, or amount of that heat, is another. Temperature thus shown by the thermometer is then but the index which marks the several changes as they are produced. In the case before us, the thermometer merely indicates the quantity or sum of units of heat and atoms of vapor present *in any given space*. After an examination of the degrees of mutual repulsion*

* Much information may be obtained on this subject from Faraday's views in his "*Researches in Electricity.*" He there observes: "The condition of elasticity upon the exterior of the gaseous or vaporous mass must be connected directly with the action of solid bodies, as nuclei, on vapors, causing *condensation on them* in preference to any condensation *in the vapors themselves.*" Again he adds: "This leads to the consideration of what are the respective conditions at the surfaces of contact of two portions of the same substance at the same temperature—one in the solid or liquid, and the other in the vaporous state, as for *instance steam and water*. It would seem that the par-

which the elementary particles of several elastic bodies exercise, Dalton observes: "It appears to me as completely demonstrated as any physical principle, that, whenever any two or more such gases *or vapors* are put together into limited or unlimited space, they will finally be arranged each as if it occupied the whole space, and the others were not present." Now *the strict application of this law is what is contended for in this treatise.* Vapor being an elastic fluid, and endowed with all the properties common to its kind, why, it may be asked, shall not this law be equally applicable to the vapor of water as to any other known vapor, each being "arranged as if it occupied the whole space, and the others were not present."

The whole question. then turns on this, whether the vapor of water acts the part of an independent gas, and retains its properties while *in a medium of water, as others do.* That it does so maintain its identity and individuality will be "as completely demonstrated as any physical principle can be."

It is not a little remarkable that Dalton everywhere considers *water* as acting the part of a vacuum, with reference to gases and vapors introduced into it, *with the single exception of the vapor of water itself.* That this exception is unwarranted will be shown by numerous proofs; among these, the most patent and self-evident is the re-appearance of the vapor itself, rising with all its properties on being liberated from the water; for we cannot draw any

ticles of liquid are in a *different relation to the latter,* to what they would be with respect to any other liquid or solid substance, if the independent action, which I have taken as a consequence of Dalton's principle, be correct." Again he observes: "It would seem that the mutual relation of similar particles, and the indifference of dissimilar particles which Dalton has established, as a matter of fact, amongst gases and vapors, extends to a certain degree amongst solids and fluids; that is, when they are in relation by contact with vapors, either of their own substance or of other bodies."

distinction between the vapor rising from water *after* the heat has been withdrawn, or *before* it.

That *gravity* has nothing to do with the mixing and diffusion of gases or vapors in the medium into which they may be introduced was proved by Dalton himself. These proofs were drawn from him in his reply to Priestly, who suggested that, "Though gases might, when mingled, diffuse themselves through one another without regard to their specific gravities, it still might be possible to bring them into contact with so little agitation as that they should remain separate."

Professor Graham, it is stated, has investigated the phenomena of diffusion with extreme precision, and has determined that "the diffusive volumes are inversely as the square of the densities of the gases." Dalton, however, has given so clear a definition of the diffusion of gases *in water*, that nothing more is wanted. He observes, "If a quantity of water freed from air be agitated with any kind of gases *not chemically uniting with water*, it will absorb its bulk of the gas." Why then shall not the same reasoning and the same law be applied to the mixing and agitating of the *elastic fluid vapor with water?* Why shall it not be said that if a quantity of *vapor be agitated with water*, it will absorb or take up the bulk of the vapor. Now, this mixing and agitating of water and vapor will be found to be literally and strictly true.

That this exception, as to vapor, has been admitted by Dalton, may be accounted for by his ready adoption of the views of his great cotemporary, Watt—namely, that vapor cannot co-exist with water when brought into contact—that, in fact, the one would instantaneously destroy or condense the other, and re-convert it into water. Nevertheless, this impression, though altogether erroneous, has been continued

down to our day. (See Section on Condensation.) That the union or mixing of vapor in water does not destroy the vital principle in either (divergence in the one, and attraction in the other), is fully illustrated even by Dalton himself. "All gases," he observes, "that enter water and other liquids by means of pressure, and wholly disengaged again by the removal of that pressure, are *mechanically mixed* with the liquid, and not *chemically combined with it.*" Now, there cannot be a more appropriate illustration of what actually and visibly takes place when vapor or steam is mixed with or injected into water, whatever may be the temperature. But, he adds further, "Gases so mixed with water, *retain their elasticity or repulsive power amongst their own particles just the same in water as out of it—the intervening water having no other influence, in this respect, than a mere vacuum.*" This is all that is here contended for. This is literally true when vapor is mixed with water. Its denial would but again raise the question whether vapor was or was not an elastic fluid; yet Dalton distinctly states, and all authorities concur in his view, that "vapor cannot, on any scientific principle, be classed in a distinct category from elastic fluids, retaining its elasticity and repulsive power among its own particles."

When, therefore, we find the indicated temperature to be the same, both *above* and *below* the surface level, in a close vessel (whatever may be that temperature), we are justified in assuming that the quantity or number of atoms of vapor from which alone the heat proceeds, pressing on or in contact with the thermometer bulb, must be the same. To suppose that the atoms which influence the thermometer *below* the surface are atoms of *water*, while those *above* it are atoms of *vapor* (and which alone could satisfy the received notions), would be to infer that each

atom of the *liquid* had the same quantity of heat as those of the *vapor*. In a word, that liquid and vapor atoms are the same as regards temperature or the quantity of heat contained in each respectively.

An illustration of this is shown when steam is injected into a vessel of water, the result, after diffusion has taken place, being an equilibrium of temperature throughout the mass. On this Dalton adds, "Experiments made upon six different liquids agree in establishing this as a general case: namely, that the variation of the force of vapors, *from all liquids*, is the same for the same variation of temperature," that is, that the elastic force of vapor is uniform, without reference to the liquid from which it is produced. The six different liquids were, sulphurous ether, spirits of wine, water of ammonia, solution of muriate of lime, mercury, and sulphuric acid.

His biographer then adds, the concluding paragraph of the memoir on absorption, as being the first announcement of his great discovery, must be introduced in his own words, viz.: "The greatest difficulty in the *mechanical hypothesis* arises from different gases observing different laws. Why does water not admit its bulk of every kind of gas alike? I am nearly persuaded that the circumstance depends upon the weight and number of the ultimate particles of the several gases, those whose particles are lightest and single being less absorbable, and the others more, accordingly as they increase in weight and complexity. An inquiry into the relative weights of the ultimate particles of bodies is a subject, as far as I know, entirely new. I have lately been prosecuting this inquiry with remarkable success."

Pursuing his inquiries, it was not until March, 1799, that he announced the true theory of aqueous vapor.

"From his grand conception," says Dr. Henry, "of the nature of mixed elastic gases and the force of steam we trace his inquiry into the tendency of elastic fluids to mutual diffusion in 1803. To the same parentage we may now trace his first vision of the atomic constituents of matter. It is impossible to peruse the essay on the constitution of mixed gases, and especially to contemplate *the plate by which it is illustrated*, without perceiving that meditation on the constitution of homogeneous and mixed elastic fluids had impressed his mind with a distinct picture of *self-repellent particles or atoms.* Thus he affirmed that homogeneous, elastic fluids are constructed of particles that *repel each other with a force decreasing directly as the distance of their centres from each other, and as a necessary sequence, that the distances of the centres of their particles, or which is the same thing, the diameters of the spheres of influence of each particle are inversely as the cube roots of the density of the fluids.*"

In these remarkable words we recognize on the one hand the distinctive peculiarity of *liquid* atoms with the force of *attraction*, among themselves; and on the other hand, the elastic fluid, *vapor*, with its force of repulsion; *two antagonist forces which cannot coexist in the same atom.* This also establishes the separate identities of the vapor atoms while in the water, which acts as a *liquid atmosphere* for their diffusion. A right understanding on this head is of the last importance as furnishing a key to the many disputed and doubtful points which still embarrass the subject.

In further illustration, Dalton observes, "there are here two very important facts, the first is that the *quantity of gas absorbed is as the density or pressure.* This was discovered by Mr. William Henry, before either he or I had

formed any theory on the subject. The other is, that the density of the gas *in the water has a special relation to that out of water.* Thus, in the case of carbonic acid, the distance within and without is the same, or the gas within the water is of the same density as without. In olefiant gas, the distance of the gas in the water is twice that without. In oxygenous gas, the distance is just three times as great within as without; and in azotic gas, it is four times. This fact was the result of my own inquiry. The former of them I think decides the effect to be *mechanical;* and the latter seems to point to the principle on which the equilibrium is adjusted."

As the plan given by Dalton, descriptive of the "self-repellent particles or atoms" is highly instructive, and much to the point in the subject before us, the state of vapor in water, a *fac similie* of it is here introduced; see Figs. 7 and 7*a*. The movements and action of the atoms of vapor in the medium of water, under the influence of mutual repulsion and diffusion, are thus symbolized and brought within the scope of our senses, and their distinct existence, apart from the atoms of the water medium in which they exist, illustrated.

Of the value of these diagrams, Dr. Henry speaking of

Fig. 7.—SIMPLE ATMOSPHERES.

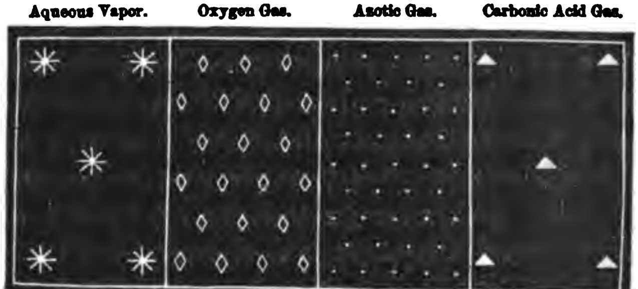


Fig. 7a.—COMPOUND ATMOSPHERES.

the molecular constitution of bodies, observes—"by the united efforts of a considerable number of inquirers, these substances have, as it were, become transparent to *the mental eye*, which traces their internal construction, and thus affords a perception of relations, which, but a few years ago, appeared utterly inaccessible to observation." Dr. Henry goes on to say "all Dr. Hofman's lectures were illustrated by elaborate atomic symbols and diagrams. Dalton was even wont to affirm that *no conception was clearly grasped by the intellect, if it could not be visibly depicted or embodied to the external sense.*"

With such authorities before us we may here safely proceed in a similar course of combined inquiry and visible illustrations—the plan adopted in these pages.

Dr. Henry observes that "the plate which is here reproduced furnishes ocular demonstration that it was in contemplating the essential conditions of *elastic fluids* that he first distinctly pictured to himself the existence of atoms."

In this figure we have a clear conception of the practical effect and process of diffusion or divergence among vapor atoms, as forming in their aggregate an *elastic fluid*, "the particles repelling one another with a force decreasing directly as the distances of their centres from each other." Dalton has also given, on an enlarged scale, the following

figure, showing by their respective radii this diffusive action, the vapor filling the entire space as if it had been a vacuum. See Fig. 8.

Fig. 8.

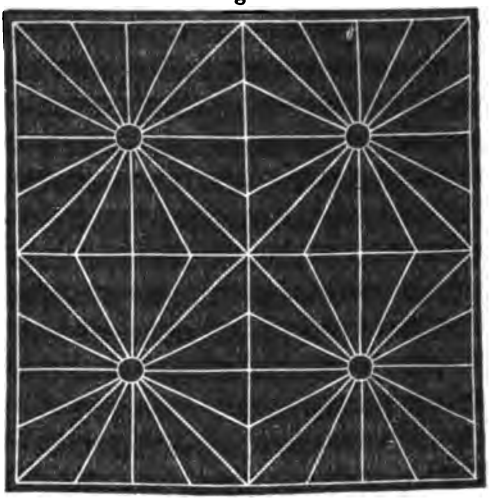
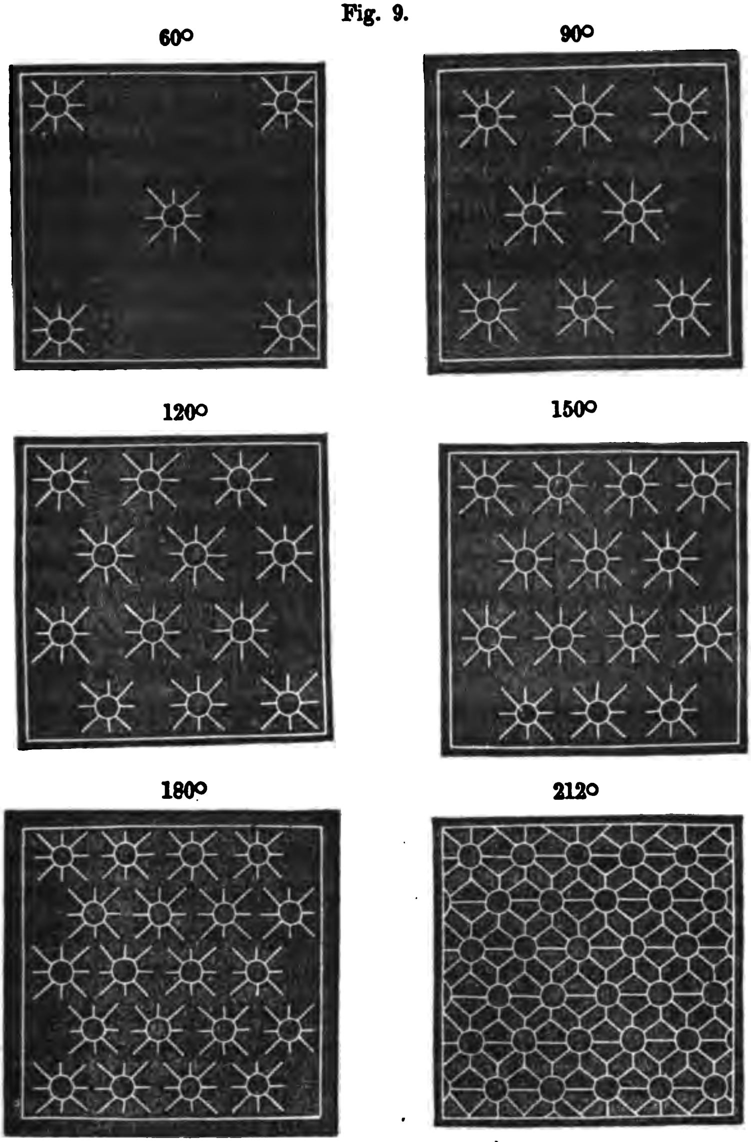


Fig. 9.



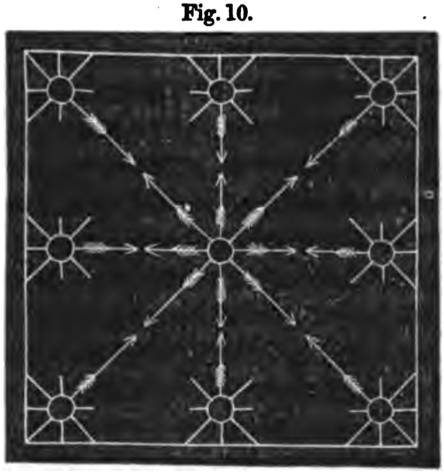
Following Dalton's mode of illustration, let the above Fig. 9 represent several masses of vapor in water, under

varying quantities, and consequently, exhibiting varying degrees of divergence, pressure, and temperature—the atoms repelling each other, and their respective distances being in proportion to the quantity or number in each given space. To avoid unnecessary detail, the vapor atoms are here represented as filling equal cubical spaces, at but six different stages or temperatures, say from 60° to 212°. At this latter, which may be called the point of ***saturation***, or the so-called ***boiling point***, the atoms are represented as close together as they can be under mere atmospheric pressure.

Now, we have only to apply the law of Dalton in reference to vapor atoms as it does to those of all other elastic fluids, and we have a practical key to the states and quantity of vapor existing in water or air, at all temperatures and pressures.

If to this, also, we add, in the words of Professor Faraday, that "each particle be a little nucleus to an atmosphere of heat or electricity," we have a clear view of the source and cause of divergence or repulsion, and the consequent

Fig. 10.



amount of pressure or elastic force. This may further be illustrated by Fig. 10, representing on an enlarged scale the nucleus of each particle—the arrows indicating the mutual repulsive force and direction arising from the atmosphere of heat or electricity surrounding it.

If then we suppose each of these six squares, see Fig. 9, to contain a cube of water, with varying quantities of steam injected into them, sufficient to raise the indicated temperature, say to 60°, 90°, 120°, 150°, 180°, 212°, we have Dalton's law fully satisfied, the water in each acting, as it were, the part of a vacuum, while the vapor atoms take their respective distances from each other.

We have already spoken of *thermo-electricity*, or the electricity derived from the accession of heat, as producing the tendency of the particles acted on to separate or repel each other. Let us now, on the Daltonian principle, endeavor, by a visible representation, to bring the conception within reach of our outward senses. Let *a a*, Fig. 10*, represent two insulated atoms of a liquid, or the two pith balls of the electroscope, negatively electrified, and consequently in close contact under the influence of attraction; let *b b* represent the same when positively electrified by the influence of heat; the result will be that, *attraction* giving way to *repulsion*, the two liquid atoms will repel each other as we see the pith balls do. The range or extent of divergence will depend on the state of the surrounding medium, or the proximity of matter in an opposite state of electricity.

Again, let *A*, Fig. 10**, represent three atoms, part of a liquid mass negatively electrified, and consequently in close contact. On being brought into connection with a charged phial, or stick of sealing-wax excited by friction, they will, as before, repel each other, as shown above—the

radii here representing the divergence, or repulsive force, and the outer circles, the range of such force. We have here so close an analogy between the repelling action of the pith balls of the electroscope, and the diverging force of vapor atoms, as fully to satisfy us that the cause of the repulsion and divergence is the same in both cases.

Fig. 10*.

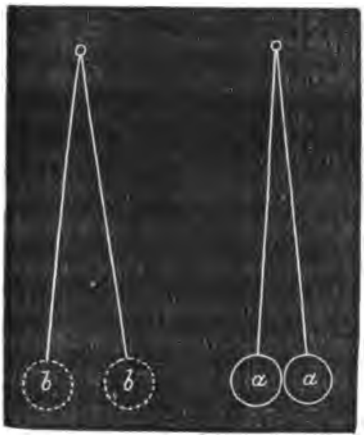
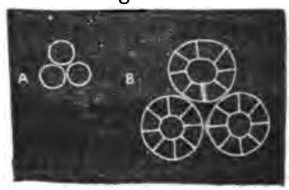


Fig. 10**.



We now proceed to the practical application of these laws of attraction and repulsion.

SECTION IV.

OF THE HEATING AND EXPANDING OF WATER.

THE recognized theory on this subject is—that water, *still retaining its liquid form and character*, absorbs heat, and expands in proportion to the quantity absorbed, up to the temperature of 212°—the amount of the expansion being then equal to $\frac{1}{2250}$th of its value. Dr. Ure estimates it at $\frac{1}{25}$th. So many proofs, however, may be adduced in contradiction to this theory, that the subject manifestly requires a further examination.

We will first consider it in reference to the properties of

COMPRESSIBILITY AND INCOMPRESSIBILITY.

The amount of compression of which water is susceptible is admitted to be so small as to justify the assertion that it is practically incompressible.

Perkins found that under a pressure of 2,000 atmospheres the amount of compression was almost *nil.* As several experimenters have insisted on some degree of compression, this may legitimately be attributed to the portion of air and vapor which it contained.

As all writers, while admitting the incompressibility of water, nevertheless insist on its expansibility, it will be advisable to refer to some of the recognized authorities, and examine the grounds on which this theory is founded.

Dr. Lardner observes: "All *solid* bodies being gradually heated from the temperature of melting ice (32°) to that of boiling water and then gradually cooled down from 212° to 32°, will be found to have exactly the same dimensions

at the same temperature during the process of heating and cooling, the gradual diminution of bulk in cooling corresponding exactly with the gradual increase of bulk in heating. Glass and other bodies, gradually heated from 32° to 212°, which undergo degrees of expansion of the solid / to corresponding *two* degrees of the thermometer, is *twice* the expansion which corresponds to *one* degree, and so on, the quantity of expansion being multiplied in the same proportion to the degrees through which the temperature had risen is multiplied." Now, let us apply this rule in the application of heat to the liquid, water, and see how strictly analogous it is, viz.: The number of its atoms converted into vapor corresponding to *two* degrees of the thermometer is twice the number (and twice the volume) that corresponds with *one* degree, the number vaporized being multiplied in the same proportion to the number of degrees through which the thermometer has risen.

Again, he observes: "The force with which a solid dilates is equal to that with which it would resist compression; and the force with which it contracts is equal to that with which it would resist expansion." This is simply an expression of the law of action and reaction, and of dilatation and compression, being correlative terms.

Now, this correspondence in *force*, being a general law of nature, must be applicable to all bodies; and what are the elementary constituents of a liquid but bodies subject to such general law?

Dilatation and compression are treated as being reciprocal and proportionate. Yet see how a positive law in physics may be rendered doubtful or negative when applied to liquids,—an arbitrary application being resorted to in order to satisfy this theory of expansion, namely, "The force with which *liquids* dilate is equivalent to that with which

they would resist compression; and as liquids are *nearly incompressible*, this force is *very considerable*." But why not apply the same terms and draw the same conclusions in both cases, namely, that as liquids are *nearly incompressible*, they must necessarily be *nearly inexpansible?* Professor Graham observes: "Regnault has recently determined the compressibility both of water and mercury with great care, and estimates the compression of mercury for each atmosphere at 3·5 millionths of its bulk, whilst he found that of water to be equal to 47 millionths of its bulk."

By the above dictum it is sought to be inferred that the resistance to expansion or dilatation, *as regards liquids*, is *not* commensurate to that of compression; in a word, that action and re-action are, in this case, not equal and opposite. We shall, however, find more harmony in Nature's laws.

If, then, these liquids be incompressible, *pari ratione*, they must be inexpansible; for if the resistance to compression cannot be overcome, neither can the resistance to expansion or dilatation.

ON CONDUCTIBILITY.

We will next test this theory of heating and expanding by reference to *conductibility and non-conductibility*. Professor Brande says: "As liquids are enlarged, and consequently rendered specifically lighter by heat, it follows that in heating *a* mass of liquid very different effects will be produced, by applying heat to different parts of the vessel containing it: *a* and *b* in the annexed Figs. 11 and 12 represent two tubes of thin glass filled with water. If we apply the flame of a spirit lamp to *the bottom* of the tube *a*, the water will *soon be heated equally throughout*, and boils, and this is the usual way of applying heat, namely, to the bot-

tom of the vessel; but if, as in the tube *b*, Fig. 12, we heat *the surface* of the water, it may be made to boil in the upper part, whilst at the bottom it will remain cold. In this case the water *becomes heated* and consequently *floats upon the colder water below, to which scarcely any heat will be communicated.*"

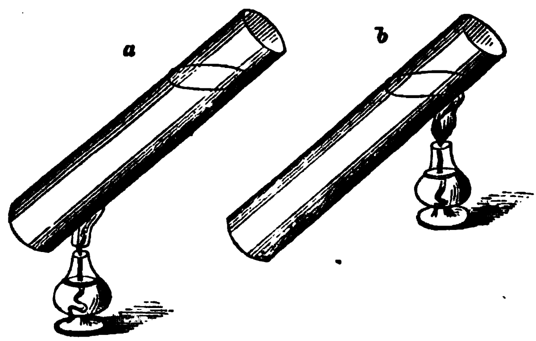


Fig. 11. Fig. 12.

Here are a series of oversights as to what really takes place, and what the experiment would have illustrated under a more extended observation. In truth, this experiment here referred to in proof that heat *cannot be communicated downwards*, may justly be used in proving the reverse.

If it were true that the water really became *heated*, and still retained its liquid form, the inference, as to expansion, would be correct. Nothing of the kind, however, really takes place. When heat is really applied to *the bottom* of tube *a*, *vapor is at once formed*, and, rising through the water, passes into the air.

Precisely the same thing takes place when heat is applied to the *upper part* of tube *b*. Here, however, the vapor passes away more rapidly from being sooner in contact with the air, and not having to pass through or be diffused in the water medium. In the tube *b*, then, so

long as the surface area of the water is adequate to the escape of the vapor produced, it will necessarily pass into the air as rapidly as it is formed. But let the heat be increased, and the case will be altogether different. It will then be found that heat *can be communicated downwards.* This, however, will not be by *conduction*, but by the ordinary process of *diffusion*, as in the above Fig. 13.

Fig. 13.

The heat here being increased by using the glass chimney of an Argand burner, instead of the spirit lamp, and having a larger tube, the vapor will be seen passing over to the opposite side in the ordinary cloud-like form, and gradually *rolling downwards, even to the bottom of the vessel.* In this case the line of influence may be distinctly observed with its wave-like form at *A*. Now, as liquids have no diffusing property or action, what descends *must be vapor and not heated water.* Had such existed, it would have "*floated upon the colder water below.*"

It may be here remarked, that in this experiment, which we find repeated in numerous works, no notice whatever is taken of the all-important fact of the generation, existence, and escape of the vapor, and which fully accounts for the absorption of the heat without the necessity of supposing its passing into the water.

This illustration, as to the non-conducting property of water, may then be referred to in proof of the *existence of the vapor* in an independent state, and its influence in causing heat to descend. In truth, were the theory above quoted practically correct, no heat could be communicated

to the lower part of marine boilers which are below the level of the furnaces.

And see the contradiction which this theory of heating of water involves. The heated water is said "*to float on the surface* of the colder portion;" and again, that *it descends* by circulating currents. Yet to float on the colder water is manifestly in contradiction to this assumed descending action.

On the non-conducting power of liquids, Professor Daniel observes:—"Liquids conduct with such difficulty that it has been doubted by some philosophers whether they be not destitute of this power. If we nearly fill a glass tube four or five inches in length, with water, and heat the upper part by a spirit lamp, we may cause the water to boil at its surface, while we hold the tube in the hand without inconvenience, the water not being able to *conduct the heat downwards.*" This, it will be seen, corresponds with the experiment above referred to, yet we have abundant proof that heat can be conveyed downwards.

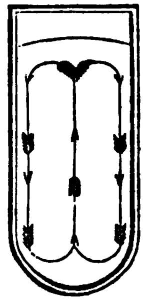
Sir Robert Kane also says:—"Liquids conduct heat very slowly: so slowly, that they were long considered to be true non-conductors,"—adding, "that when any portion of the liquid is *heated, it expands,* and becomes specifically lighter, *ascends in the mass and is replaced by colder and heavier portions, which being in their turn heated, ascend also, and thus generate a circulating current, as in* Fig. 14, *by which every particle of the liquid is brought in succession into contact with the source of heat, and the resulting temperature quickly and uniformly gained.*" *

Here we find one of the most accurate experimenters

* "Elements of Chemistry," by Sir Robert Kane, M. D., M.R.I.A., Professor of Natural Philosophy to the Royal Dublin Society, Professor of Chemistry to the Apothecaries' Hall, Dublin, and President of the Queen's College, Cork.

falling into the common error as to the heating of liquids, and the *circulating currents* being *the cause of the uniform temperature* of the mass. Let us then examine the process more closely.

Fig. 14.



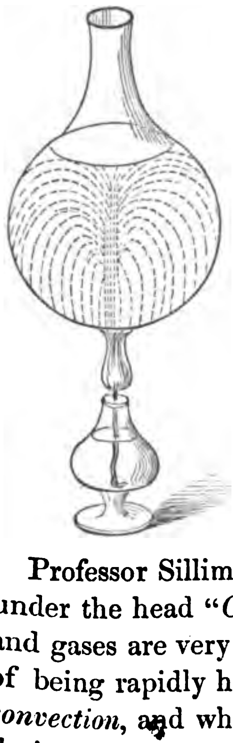
When heat is applied beneath a vessel of water, it is true a movement upwards is created; it is, however, of a soft, diffusive character, rather than indicating direct ascending and descending currents. It is of the cloud-like diffusing character, and perfectly distinguishable as already described. (See Vaporization, Fig. 3.) It is only when about the temperature of 180°, or when incipient ebullition begins to act, that currents are created and become available. (See Section on Ebullition.) It is manifest then that the homogeneous state, as to temperature, and which is perceptible from the commencement, must be due to some other cause. Besides, it would be physically impossible that any but the smallest quantity of the liquid atoms could have been so rapidly "brought in succession into contact with the source of heat" at the bottom of the vessel, as to produce this uniformity of temperature.

Dr. Read repeats the same theory of ascending and descending currents, but makes no reference to the movements of the *vapor* in the body of water.

Professor Graham, unquestionably one of the highest authorities, adopting the same theory of currents, observes: "The diffusion of heat through liquids and gases is effected in a great measure by the motion of these particles among each other. When heat is applied to the lower part of a mass of liquid, the heated portions become lighter than the rest and ascend rapidly, conveying or carrying the heat through the mass of the fluid. In a glass or flask, for in-

stance, with which a small quantity of any light insoluble powder has been mixed, a *circulation of the fluid* may be observed upon the application of the flame of a lamp to the bottom of the vessel, the heated liquid rising in the centre of the vessel, and afterwards descending near its sides, as represented in the annexed Fig. 15. But when heat is applied to the surface of a liquid, this circulation does not occur, and the heat is propagated very imperfectly downwards. It has even been doubted whether liquids conduct downwards at all, or, indeed, in any other way than by conveying it as above described."*

Fig. 15.



When such an authority as Professor Graham thus repeats the same views, as to the ascending and descending currents causing the heat to be conveyed to all parts of a liquid mass, and accompanies them with so precise an illustration, it behoves us to be cautious, and to be assured that we have satisfactory grounds for questioning his authority.

Professor Silliman,† adopting the same theory, observes, under the head "*Convection in liquids*": "Although liquids and gases are very poor conductors of heat, yet they admit of being rapidly heated by a process of circulation called *convection*, and which depends upon the free mobility of their particles. The particles of liquids and gases in immediate contact with the source of heat becoming warm,

* "Elements of Chemistry," by Thomas Graham, F.R.S., Professor of Chemistry in University College, London.

† "First Principles of Physics, or Natural Philosophy." By Benjamin Silliman, M.A., M.D., Professor of Chemistry in Yale College, New Haven. 1859.

and also specifically lighter, rise, and, moving away, make room for others; this is continued until all the particles attain the same temperature. The circulation just mentioned may be rendered visible by heating water in a flask containing a little bran or amber, over a spirit lamp, as shown in the annexed figure. The particles of liquid at the bottom of the vessel, where the heat is applied, rise, and other particles of colder liquid come in below and supply their place. Thus two systems of currents are formed. In the centre of the vessel, ascending currents of hot particles and descending currents of colder particles flow down the sides; this circulation continues until the whole mass has attained the same temperature."

Professor Silliman, in his work just published, 1859, appears to have merely followed Professor Graham, and has given identically the same illustration of the flask as in Fig. 15.

It is here only necessary to say that this theory of ascending and descending currents being the means of bringing all the particles into immediate contact with the source of heat, and thus effecting a uniform temperature in the mass, is altogether erroneous. The following experiments, which may be easily performed, will afford ample evidence, 1st: That the uniformity of temperature has no reference to these currents. 2nd: That it is solely attributable to the diffusion of the particles of vapor through the liquid. 3rd: That the falling of the motes of bran or amber are exclusively the result of their *gravitation*, until the process of ebullition (hereafter to be explained) begins, and which gives a mechanical force to their ascent by which *circulation* is produced.

And see how liable we are to be deceived in the question of these currents. By the mode of conducting this experi-

ment, the flame of the lamp, as in Fig. 15, is applied merely to the centre of the bottom of the vessel, and, consequently, a large and rapid generation of vapor is created in that locality. Whatever light particles come in the way of this ascending vapor are carried upwards with increased force. These, however, on reaching the top of the liquid mass, are passed to the sides, out of the upward movement, and thence descend by *their own gravity;* their motion downward, however, is so slow, and even irregular, as unmistakably to show that it is not the *water itself* that is passing downwards, but that the *motes are passing through it.*

There is nothing, then, more remarkable than this acquiescence without inquiry or proof on the part of writers of high standing and authority in this theory, which attributes to these currents the progressive uniformity of temperature which pervades a mass of water from the moment heat is applied to it. So little doubt existed in the writer's own mind on the subject that he readily adopted the same theory, following Professor Brande, when examining the question of *circulation in boilers*, in his treatise on "The Combustion of Coal."* That ascending and descending currents do ultimately prevail is as certainly true—equally so that they are mainly instrumental in effecting *circulation*—in which sense the theory was there adopted. Subsequent experience, however, distinctly proved that they have no reference to that homogeneity of temperature which prevails long before these circulating currents begin, and which are solely attributable to the action of ebullition, as will hereafter be shown.

In all these statements we see every thing is assumed—the heating and expanding of the water, the ascending and

* "The Combustion of Coal Chemically and Practically Considered." By Charles Wye Williams, A.I.C.E. John Weale, London.

descending currents, and the supposed contact of every particle of the water with the bottom of the vessel.

Gmelin himself observes—"The communication of heat from the upper to the lower part of a liquid takes places so slowly that Rumford absolutely denied the existence of conducting power in bodies of this class;" thus attributing the uniformity of temperature to these currents, the existence or possibility of which may be indisputably disproved. On this the following experiments may be considered as conclusive:

Five pounds of water which had lain some time in an open beaker had collected a quantity of brown light flocculent deposit on the bottom, and which the slightest motion would disturb. As this deposit covered entirely the bottom, it was impossible for any fresh body of water to reach the glass and receive the heat from it without disturbing and displacing such sedimentary matter.

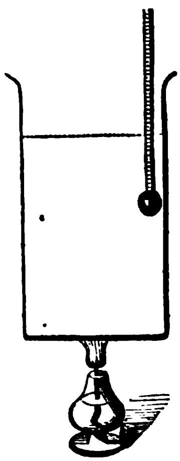
Heat being applied beneath and *against the whole of the bottom, in one minute* the thermometer indicated an increase from 56° to 57°, and continued for some time rising at the rate of about one degree per minute. Subsequently, the temperature *throughout the mass* increased at a more rapid rate, but always with remarkable uniformity. Now, to suppose, in the words of Sir R. Kane, just quoted, that in such a body of water, and in a *single minute of time*, "every particle of the liquid is brought in succession in contact with the source of heat, and heated throughout," was physically impossible. The same may be said of the continuing uniformity and homogeneity of the liquid mass, as such would involve the necessity of every atom of the water coming momentarily and in succession into contact with the bottom.

During this time whatever movements of the light par-

ticles of the sediment took place upwards were due to the vapor rising in the cloud-like form through the mass in the process of diffusion. No descending currents, as mentioned by these writers, taking place—the homogeneous character of the body, as *to temperature*, was manifestly resulting from some other cause, which had been overlooked.

In further illustration—5 lbs. of water at 60° were placed in a large beaker, and heat from a spirit lamp, as shown in Fig. 16, was applied to *the centre only* of the bottom of the vessel, to insure the formation and rising of the vapor in that locality, a thermometer was suspended so as to be out of the way of the centre ascending movement. Before one minute had passed the thermometer, as before, rose from 60° to 61°, and continued for several minutes rising at the rate of one degree per minute. Yet it may unequivocally be said that no descending currents of the liquid took place. The action on the thermometer bulb was then owing exclusively to the diffusion, *vertically* and *laterally*, of the newly-formed vapor.

Fig. 16.



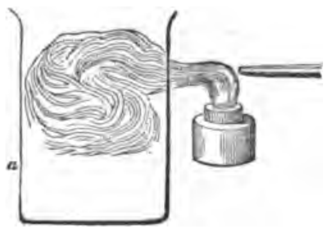
Now, the value of this experiment consists, not so much in disproving the theory of *descending currents*, as that it furnishes conclusive evidence both of the existence of the vapor in the water, and its diffusive action throughout the mass.

The following experiment is still more conclusive, both as to the *non-conducting* property of water, and the *descending* action of the vapor, and that such is the true cause of the uniformity of temperature throughout.

A large blowpipe was applied to the flame of a gas-

burner by Herapath's apparatus. The flame was projected against a beaker of water, about two inches in diameter, and made to spread against the glass, as shown in Fig. 17. Ebullition soon took place, and the mixed vapor and water were seen rolling over and *descending*, as here shown. By dipping a thermometer into the water, the line of influence of the hot stratum at *a* was clearly visible. This line may also be seen, in a *wavelike* form, descending until it reaches the bottom. It is here advisable to cover the vessel partially, to prevent the too rapid escape of the vapor. This may be said to be the process by which the water in marine boilers becomes influenced throughout, and circulation effected, until ebullition has commenced, the mechanical force of which further promotes that circulation.

Fig. 17.



We will now proceed to consider and test the remaining part of the theory as to

EXPANSION.

On this, as on many other points, we are in general too much in the habit of yielding to plausible inferences, rather than be at the trouble of inquiry. Inferences, which would not stand the test of examination, are thus adopted and recognized until they obtain the credit of established facts. Of this the theory of expansion of liquids furnishes an apt illustration.

Gmelin, now considered as of the highest authority, adopting the same views, observes: "When heat is communicated to the bottom of a liquid, it *diffuses itself quickly and uniformly throughout*—not, however, by conduction, that

is, by radiation from particle to particle, but in consequence of *currents in the liquid itself.* The lower portion, which is heated, and *thereby expanded*, ascending, while the colder and heavier portion sinks."*

Here we have from this high authority the direct assertion, not only that the lower portion of liquids are heated by descending currents, but that, on being so heated, the liquid *expands* and ascends in the mass.

Thomson, in common with many other writers, has labored in endeavoring to reduce the amount of this expansion to some general principle.

Finding, however, that no general law exists respecting the expansibility of liquids, he is forced to conclude that "every liquid has an expansibility peculiarly its own, and which, therefore, may be called *specific*;" adding that "all that can be done is to determine the degree of expansion which each liquid undergoes when subject to a given temperature." Now, this very irregularity is alone sufficient to show that we are not on the right road to a satisfactory solution of the difficulty. There is, however, so much of harmony in all nature's laws that we may safely infer the existence of some sufficient principle from which uniformity may be deduced.

All writers concur in saying that *vapor* and *air* follow the same law as gases or vapors of all kinds. Pursuing this inquiry, Thomson says that "Dalton and Gay Lussac, by keeping the gases experimented on *dry*, were enabled to discover that all gases experienced the same augmentation of bulk when subjected to the same augmentation of temperature." Hoping to find a similar coincidence in liquids, the subject was pursued with great labor, forgetting,

* "Handbook of Chemistry." By Leopold Gmelin. Printed for the Cavendish Society.

however, that the atoms of gases, vapors, or other elastic fluids have no *fourth state*, into which they may be resolved by additional temperature; whereas liquid atoms, by heat alone, become, virtually, atoms of an entirely different class of bodies, and possessed of essentially different properties—as different in fact as are the elements of water—oxygen and hydrogen—in their separate states as *gases*, and their combined state as *liquid water*.

Baffled in the attempt, Thomson comes to the conclusion that "liquids differ from gases in this, that their expansibility is *not uniform*, but that the *rate of expansion* increases with the temperature, and is, therefore, the greater, the higher the elevation at which a given quantity of heat is added to them."

He saw the difficulty of reducing the *rate* of expansion in liquids to the law which regulated that of gases. Yet, by applying the law of the *quantity of vapor* present in any given body of a liquid, a sufficient solution of the rate of expansion would have arisen; and *this* doubtless will hereafter be determined.

"Liquids," he adds, "differ from gaseous bodies in a very remarkable circumstance. The particles of gaseous bodies *repel* each other, but those of liquids *attract*, as is evident from their collecting together in spherical drops. It is obvious that this attraction between the liquid particles must act as antagonist to the expansion produced by heat." This essential difference, then, clearly justifies the placing them in different categories, and destroys all grounds of comparison.

Dr. Lardner observes—"The same vessel will hold a greater quantity of cold than hot water. If a kettle filled with cold water be placed on the fire, the water, when it begins to warm, will *swell* and flow from the spout until it

ceases to expand." If by expansion the enlargement of the gross bulk of the water only is implied, this is true enough. But such expansion is the result of elementary atoms being successively converted into vapor. In no other sense does the water *swell.* And in *that* sense it never ceases to swell all the time heat is applied, until the point of saturation is reached.

Again he says,—"Since the magnitude of any body changes with the heat to which it is exposed, and since, when subject to the same calorific influence, these dilatations and contractions, which are the constant effect of heat, may be taken as the measure of the physical cause which produces them—"

This is doubtless correct when applied to individual bodies, among which the *constituent particles* of liquids may be classed, but not to the *aggregate of those bodies.* Here then lies the main source of error. If water were a body to be dealt with *in bulk*, as a ball of iron or lead, and capable of receiving and *conducting, from atom to atom*, successive increments of heat, without any change in its *status of liquidity*, we might, in such case, correctly infer its expansion. But water, or indeed any liquid, has not that power or property of conduction among its constituent particles as metals or solids have, and consequently is incapable of expansion in the sense of such bodies. Besides, heat, that invisible and imponderable agent, knows nothing of the mass of contents of the vessel. It deals only with the *individual atoms* of which the mass is composed, whether liquid or solid, and with which it comes into contact. When also we consider how nature in its wonderful economy apportions the combining volumes, weights, or other properties of matter, there can be no disproportion between atoms of liquids or solids, and units of heat. Their union is but part of the immutable law of nature.

stamped on matter of all kinds. Each atom has not only its specific duty to perform, but the faculty of performing that duty; none will be tried and found wanting. The power of the wind is but the sum of the powers inherent in each individual atom. So of the waves; or a crowd of human beings. Pressure, or power *in the mass*, is then but that of accumulated individuals or atoms. It is to these, then, and their respective properties that our inquiries should be directed.

As water, or its constituent particles, cannot undergo any change, physical or dynamical, without some sufficient cause, liquid particles at the temperature of 32° must continue at 32° until they have each received their equivalents of heat by which they lose their *status* of liquidity. They are then, however, no longer liquid atoms—they are absolute atoms of vapor. On what grounds then can we say that liquid atoms are heated or expanded, and still retain their liquid form and properties? Such an hypothesis would be contrary to the evidence of facts. To say that water can be a recipient of heat, or be expanded, while it retains the liquid state, and is also *a non-conductor of heat*, would involve a physical solecism, irreconcilable with reason and common-sense. Let us now reduce this to practice, and see how it will stand.

When we inject a small portion of steam, say an ounce weight, into a pound weight or 16 ounces of water at 60°, we find the indicated temperature of the compound will be about 100°. By the ordinary theory, the water would be said to be heated to that degree. But how can this be reconciled with the *non-conducting* property of water?

For let us suppose this ounce weight of so-called "heated water" to be diffused through the colder mass, how is it to impart its heat to the numerous atoms composing the

16 ounces of this liquid? Let us go further, and suppose that the ounce weight of steam contained 100 atoms. On their injection into the body of cold water they mingle with, say, 1,600 liquid atoms. Are we then to suppose that each of the 100 vapor atoms became divided into 16 portions to raise the 1,600 to the uniform temperature of 100°; or how are they to convey heat to each?—*conductibility being out of the question.* The homogeneous temperature of the mass would imply, either that each atom of steam was surrounded by 100 atoms of the liquid, or that a unit of heat had been imparted to each of the 1,600.

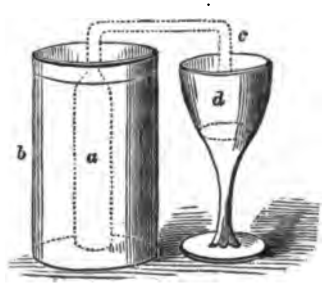
But let us examine this still closer. An atom, or body, which cannot *conduct* or convey its heat to another involves an inability to *receive* or absorb it. If atom A has received a unit of heat, but is incapable of conducting it to atom B, what is to become of it? If A cannot pass it to B, neither can B receive it. The absence of the power of conduction, then, strictly implies the impossibility of receiving—and, consequently, of being heated or expanded.

Dalton, generalizing, in treating liquids as if they were distinct bodies, falls into the same dilemma. "The changes in magnitude which a body suffers by changes in the heat to which it is exposed are called changes of temperature." This is also correct when applied to *solids* in which heat is conveyed by conduction from atom to atom. Applied to *liquids*, the question is again raised—What are the bodies to which heat is applied, and which suffer such "changes in magnitude" and temperature? Certainly not the mass or *gross body* of the liquid, but the separate and physically distinct particles, of which Dalton himself informs us all liquids are composed. It is of these, then, that "the variations in magnitude may be taken *as the measure of temperature.*"

It is not a little strange that writers who so well define the relations of solid bodies, in reference to heat, should thus deal with liquids as if they were integers of simple bodies, and not see the difference, practically, between them. Further illustrations in support of the assumed expansion of liquids, as given by writers of authority, may here be referred to, and it is important that they be examined with the view of detecting the cause of the errors they exhibit.

Sir Robert Kane observes:—"To measure the amount of expansion in liquids, *a*, in the annexed figure 18, is a glass tube, the neck of which is very narrow, and bent, as here described. The tube is to be completely filled at the lowest temperature, and weighed. It is then to be placed upright in a cylinder of oil or water, *b*, to which heat may be applied. The liquor expanding as its temperature is raised, the excess of the volume flows out of the cylinder beak *c*, and may be collected in *d*. When the apparatus has been brought to the highest temperature required, and that all further expansion has ceased, as is known by ***no liquid passing out at c***, the tube is to be removed from the bath, and, when again cold, accurately weighed. The loss in weight is the quantity of liquid that has been expelled, and this compared with the whole original quantity, gives the *proportion of expansion.*"

Fig. 18.



Here the expansion of the liquid is assumed as a positive law or fact. The liquid is said to "expand according as the temperature is raised." With more correctness it should be said, that vapor is formed in the same proportion as heat is taken up, and the bulk expands in the same ratio,

—the thermometer indicating a commensurately-increased temperature. Again, "When all further expansion has ceased, as is known when no further liquid passes out." Now, expansion never ceases as long as heat is applied. The *several atoms* of liquid, as they successively receive heat, are *expanded* into atoms of vapor, and, being respectively enlarged, occupy more space, producing an enlargement of the gross volume, a portion of which necessarily flows out as rapidly as the new vapor continues to be formed.

There is then no difference whatever between the formation and expansion of the first and the last atom of vapor.

Professor Brande gives a similar illustration, and draws a similar inference, namely:—that the proportions of expansion of the liquid are estimated by the amount of overflow. The important point, however, still remains undetermined as to *the cause* of the increased volume of the mass. Here, then, lies the oversight and consequent error. Had no part of the liquid been converted into vapor, and had none really existed in it, the inference would be legitimate, that the enlargement of the gross volume was due to the expansion of the water, though *still retaining its liquid form.* When, however, we not only see the vapor moving about in the water, but escaping into the atmosphere from the moment the heat is applied, we have proof of the formation of vapor, and of the gross enlargement of the mass due to the presence of those vapor atoms and the larger space which each individually occupied. The inference is logically correct, and no physicist can disprove it.

But it may be said that the difference is merely one in terms; that the liquid atoms have truly absorbed the heat. This would be to assert that liquid and vaporous atoms

were the same—that the difference was merely nominal—confounding the essential properties of attraction or repulsion due to each respectively.

In all these assumed experimental tests we see the main oversight consists—

1st. In ignoring the formation of vapor as rapidly as heat is applied.

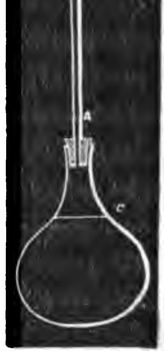
2nd. In overlooking the existence of the vapor atoms in the body of the water, and their diffusion throughout the mass.

3rd. In assuming the liquid atoms to be the recipient of the heat without any change in their *status*.

4th. In overlooking that *vapor* atoms, being necessarily individually larger than the *liquid* atoms, from which they were formed, fully accounts for the gross enlargement of the mass.

The following is a simpler and more satisfactory experiment, exhibiting the same results. Insert a glass tube, Fig. 19, about three feet long, into the neck of a bottle, fitted steam-tight in the cork. Into the bottle put as much water, say at 60°, as will bring the level to *A*; both water and bottle being accurately weighed, place it over a lamp, and let it be raised to any required temperature indicated by the level, *B*, in the tube. The difference between the levels, *A* and *B*, in the tube will then be the measure of the enlargement of *the gross volume* of the water. This would be a correct test of the *expansion of the water* under the prevailing theory, provided the vessel *contained nothing but water*. If, however, it be shown that vapor has been formed, and remains in it, the infer-

Fig. 19.



ence must be that that was the true cause of such enlargement. To test this fact, let it be immediately poured out into a heated flat dish, to give a large surface for the escape of the vapor, and which will be seen to rise in large volumes into the air, each atom necessarily carrying with it its own heat. On the vapor having all escaped from the water, and the mass consequently returning to its initial temperature of 60°, and being poured back into the bottle, both bulk and weight will be found to be reduced,—the former to the level at *C*, and (assuming the temperature to have been raised to 212°) the loss of weight will be at the rate of two ounces for every pound of water.

Dalton himself, while adopting the theory of expansion, has overlooked the existence of the vapor in the water. "All homogeneous liquids, as mercury and water, expand from the point of congelation or greatest density, a quantity always as the square of the temperature from that point." Now, that heat should be disposed of in expanding the water, is a direct negative of its application in converting it into vapor which, we have seen, carries that heat away. It cannot *remain* in the liquid atoms and *go off* in vapor at the same time. Expansion of a liquid and formation of vapor, by the same heat, are then irreconcilable and contradictory processes. Nevertheless, he is even particular in describing the precise *quantity of vapor* escaping at varying temperatures; thus:—"The comparative loss from evaporation between 212° and 138° were first ascertained. At 212° it amounted to between 30 and 45 grains, and at 164°, it was 10 to 16 grains per minute."

Here the assumed heating and expanding of the liquid is directly refuted by accounting for the disposal of the heat in the escaping vapors;—for if 30 to 45 grains of vapor escaped from the water at 212° to 138°, where could

that vapor have previously been but in the water out of which it rose? Instead of the heat being employed in expanding the liquid particles, it must have gone to the conversion of the 30 to 45 grains of vapor per minute. All through, this theory of expanding liquids leaves the question untouched as to how and where the vapor is formed. We are not told how an *expanded liquid* could be enabled to leave the vessel and rise into the air; or, by what miracle it could assume the state of an elastic fluid *without any further accession of heat.*

Dr. Ure says, *on the verge of* 212°, the volume of a cubic inch of water was enlarged $\frac{1}{23}$th, but at 212°, to that of a cubic foot, or 1,728 cubic inches.

In such case this greater expansion must have been *sudden and explosive*, the intermediate stages from 32° to 212° being altogether overlooked. Yet, during all these stages, we have absolute proof of the continued formation and escape of vapor.

"The attraction among the liquid atoms," says Thomson, "must act as antagonist to the expansion produced by heat."

Here we have a refutation of the expansion theory in the very words used in its assertion, which imply that attraction and expansion *were co-existent;* in other words, that bodies were under the influence of attractive and separative forces at the same time.

Why, then, have recourse to this abnormal theory in accounting for the enlargement of the gross volume, when we have a more rational and scientific solution in the presence of vapor atoms in the ratio of the heat received—the sum of their individually increased volumes giving a satisfactory reason for the gross enlargement. On the other hand, the heating and expanding theory involves the still

greater anomaly of assuming the conversion of liquids into vapor by a *cooling* rather than a *heating* process, namely, by exposing it to the colder atmosphere. Feeling the difficulty of giving a satisfactory solution of these anomalies, writers have either passed them by, or have suggested merely speculative theories.

Correctly speaking, then, there can be no such thing as heated or expanded water, or other liquids, even as regards mercury, which only follows the general law. In ordinary language, and as a mere conventional term, we may speak of water being heated and expanded, and of having an increased temperature. When, however, we are treating the subject in a scientific point of view, and with reference to the strict nomenclature adopted by chemists, we should avoid whatever may be unwarranted, as but tending to confusion, and this may lead to serious complications. Water may be spoken of as being mixed *with vapor*, as *with air*, and its variations of temperature described; that temperature, however, should be attributed to its right source—namely, the quantity or number of vapor atoms then present in any given space—these being the true and only source of dynamic effect.

On the whole, then, we have sufficient to justify the following conclusions:—

1st. That water, or other liquids, being incapable of *compression*, are equally incapable of *expansion*.

2nd. That water, being a *non-conductor* of heat, must also be a *non-recipient* of it.

3rd. That as it cannot be heated or expanded, and still retain its liquid form and properties, it cannot be thermometrically affected.

4th. That its enlarged volume is attributable, not to any

measure of expansion *as a liquid*, but to the presence of vapor in it, in the state of an elastic fluid.

5th. That this condition is in entire accordance with the recognized laws of all elastic fluids.

The respective properties of liquid and vaporous atoms, as regards the changes they undergo by heat, from the liquid to the vaporous state, may be thus described:

LIQUID ATOMS.	VAPOR ATOMS.
1—Gravity.	1—Gravity.
2—Latent Heat.	2—Latent and Sensible Heat.
3—Mutual Attraction.	3—Enlarged Volume.
4—Mobility, *inter se*.	4—Increased Temperature.
5—Non-Conductibility.	5—Mutual Repulsion.
6—Incompressibility.	6—Diffusion or Divergence.
7—Inexpansibility.	7—Conductibility.
8—Negative Electricity.	8—Compressibility.
	9—Expansibility.
	10—Positive Electricity.

SECTION V.

ON THE BOILING POINT.

PREPARATORY to a consideration of the practical application of the terms "ebullition," or "boiling point," it will be advisable to examine the true relation which heat bears to water in its progress to that point.

In a recent publication on the laws of steam, the writer opens the subject by observing:—"It is now almost a century since Dr. Black announced his discovery in heat, and the laws have ever since been the subject of investigation and experiment. We have learned much about the phenomena of heat since then, but the *relations between the pressure, the density, the heat, and the temperature are yet undetermined.*"

No light, however, is likely to be thrown on the subject by an examination which begins by adopting, *without inquiry*, the common but erroneous theory that water becomes the recipient of heat, although still retaining its liquid form, and in assuming, also without inquiry, that the relations between the pressure, the density, the heat, and the temperature, are influenced by separate laws.

The following extracts will show how the writer has adhered to these views, namely:—"Water *retains its heat only under pressure.* If the pressure be released, the heat which it is then unable to retain, is carried off by the formation of steam, until the increasing pressure and the decreasing heat are again in equilibrium."

With equal regard to fact might it be said that a pound

weight of *shot* retains its heat *only under pressure.* In truth, pressure has no more relation to the chemical absorption or retention of heat by the atoms of water than by the grains of a body of shot.

Again, "To retain a certain quantity of heat in water requires a pressure which increases directly as the 4⅛ power of the number of the units of heat. That pressure is maintained on water *by its own steam*, therefore, when the heat and pressure are in equilibrium, which is the case when the steam attains the pressure due to that temperature, the pressure of the steam will be measured by the 4⅛ power of the units of heat in water."

This theory appears based on the double mistake, 1st, of assuming water to be a *body* in the sense that we speak of a body of lead, rather than as an aggregate of separate bodies, say of a given weight of shot; and 2ndly, that it is only when it can *retain no more* that the heat goes to the formation of steam.

Were that the case, how, it might be asked, are we to account for the formation and visible appearance of steam, from *the moment the heat is applied*, and while the indicated temperature of the mass is but a single degree in advance from the starting point?

Again, when it is said that "The pressure on water is maintained by its own steam," it implies not only a varying degree of pressure *above* and *below* the surface of the water (which is opposed to fact), but that the water and the steam above it are antagonists as to pressure. Now the uniformity of temperature which prevails *in the water*, and *above it*, in close vessels, ought to be conclusive as to a corresponding pressure, or, what is the same thing, a correspondence in the *quantity* of vapor in both places, in excess of saturation. If this were not the case, Dalton's law of

the water acting the part of a vacuum to elastic fluids would be erroneous.

The number 212° is said to indicate the maximum temperature of water under atmospheric pressure. This, however, has been already shown to be exceptional, seeing that even a much higher temperature may be reached *without ebullition* or any increase of pressure. We have still then, to ascertain the true relation between 212° and the boiling point.

We must here deal with the subject analytically, that we may understand and apply it practically. In this way Dalton and Davy proceeded, and were enabled to throw so much light on the constitution and characteristics of matter, and produce those results which have raised chemistry to the rank of an exact science. We have then to consider the matter of water not *in the mass*, or as a single body, but as an aggregate of bodies, the constituents of which, and their several relations to heat, are the direct object of inquiry. In this way we are led to examine the action and influence of *the smallest elementary portions of the one with the smallest elementary portions of the other.* Dalton rightly observes:—"All bodies are constituted of a vast number of extremely small particles, or atoms, bound together by a force of *attraction.* Besides this we find a force of *repulsion.* This is now generally, and I think properly, ascribed to the agency of heat."

An inquiry into the constitution of the matter of water in reference to heat and temperature, should then have reference, not to the mass, but to its elementary molecules or atoms. We know that it must be with these, as *independent bodies*, that the heat has combined, and that the result of such individual unions must be, not that the whole body has been heated, but that such heat has been

confined to the individual particles which have been affected by it and converted into vapor; just as it could not with truth or relevancy be said that the entire of a regiment in the barrack were diseased and equally influenced because some few, more or less, of the body, had been affected by *coups de soleil.* We must never lose sight then of the fact that a body of water or any other liquid is as much, to all intents and purposes, an aggregate of individuals, as an army or a crowd.

We are not here inquiring into the nature of heat, but into the effect, physically and dynamically produced by its absorption or union with the atoms of the matter of water. If then, as is the case with all other descriptions of matter, *equivalent determinate* quantities are essential in producing given results, we have no reason for doubting that the same law applies to heat, in its several combinations with the matter of water. We know to a physical certainty the gaseous constituents of each elementary atom of water; we have then no rational grounds for objecting to treat of such atoms in their respective unions with heat.

A preliminary word may here be said on the subject of the "*unit of heat.*" This is generally taken as being represented by a degree of the mercurial thermometer scale. Professor Rankine observes:—"For the purpose of expressing and comparing quantities of heat, it is convenient to adopt as an *unit of heat*, or *thermal unit*, that quantity of heat which corresponds to some definite interval of temperature in a definite weight of a particular substance. The thermal unit employed in Britain is the quantity of heat which corresponds to an interval of one degree of Fahrenheit's scale in the temperature of one pound of water at, or near, its temperature of greatest density."

This may be a conventional and convenient rule in a

commercial point of view; but, scientifically considered, by what law, or on what principle, are we authorized in assuming each degree on that scale to be commensurate with a definite unit of heat? The graduation of the mercurial thermometer is a mere arbitrary process; and, looking to the extreme divisibility of matter, we are equally justified in assuming that countless myriads of units of heat are required to produce the change corresponding with each single degree of the mercurial scale. We cannot, it is true, speak of atoms of heat, imponderable as they are; yet, in a strictly scientific inquiry into the relative quantities absorbed and brought into union with equivalent quantities of water, we are justified in speaking of it as in *divided portions*. We may then allude to them under the term *doses* or *units of heat*, and their reference to increments of temperature.* This narrows the question, and warrants our speaking of one or more *units of heat* in combination with one or more *atoms of vapor*. Without some such synthetic process, all experimental researches must continue vague and unprofitable. By this atomic process we are enabled to decide, chemically and physically, on all combinations of matter. Under this system of equivalent atoms or quantities we are enabled to understand how, and why, the air we breathe, and by which life is sustained, is identical, in its elements, with that most destructive compound *nitric acid*, by which animal life would be instantly destroyed; and that the only difference between these life-sustaining and life-destroying combinations is one of *mere definite proportions* or equivalents.

* "Most bodies are susceptible of three states of existence; namely, the solid, the liquid, and the elastic, or vaporous; and all these are affected by the introduction of different *doses of caloric*."—*Rees. Cyc.*, *Condensation.*

By a process of experimental investigation, inductive of credibility, we shall be enabled duly to appreciate and determine the equivalents of heats in combination with liquid atoms, and the further important differences, physically, dynamically, and even electrically, between them and their resultant atoms of vapor. Without some such process we might as rationally expect to have a thorough insight into the nature or composition of matter, or the anatomy of the human frame, by merely observing the motions of men, or of liquid bodies *in the mass*, as to suppose we could obtain a knowledge of the relation which heat bears to liquids or vapors by merely observing the currents or other movements of a mass of boiling water.

To understand the anatomy of water in combination with heat in any of its states, we must do as anatomists or chemists would. We must, with a microscopic spirit of inquiry, look into the organization and mutual action of *units of heat* on the constituent particles or *atoms of the water.*

Heat acts on, and vaporizes the liquid atoms in the human frame in the same way, and under the same immutable law as it does in liquids, in any other form or phase. To call it exceptional that the thermometer should indicate no higher degree of temperature than 212°, is as irrelevant to the cause of that temperature as it would be to characterize it as exceptional, that the blood in the animal economy should not exceed 98 degrees, notwithstanding the continued additions of heat realized on each inspiration.

In liquids, when the thermometer indicates 212°, further increments of heat are absolutely available, not as influencing or increasing the temperature in the mass, which they could only do *by remaining in it*, but as generating further atoms of vapor; these, however, being no longer retainable

by the laws of diffusion, and under mere atmospheric pressure, rise, and escape as rapidly as they are formed, each carrying away its respective equivalent of heat. It is then not the quantity of heat that is influenced by the pressure, but the quantity, or number of atoms of vapor in the mass which influences that pressure. Each individual atom of a liquid when brought into union with an individual unit of heat, may be considered as a *distinct entity, or substance*, and no doubt bears a given relation to *temperature*, although imperceptible by our powers of vision, or measurement. These facts are well understood by chemists, yet all continue to regard liquids as separate bodies or integers, rather than as aggregates. These generalities must be abandoned, and we must look at water in its elementary particles as we would at those of other descriptions of matter. The necessity for this mode of proceeding will be the more apparent when we consider that the quantities communicated are divisible into two distinct classes, namely, *latent* and *sensible*, or, more correctly speaking, *statical* and *dynamical*—the former being identified with the *status* of water as a *liquid*, and producing no thermometrical effect whatever, while the latter, however limited may be the quantity, has its dynamic influence exhibited by its action on the thermometer, or other body (if capable of conduction), with which it may be brought into contact.

Looking more analytically at the process, we find that the smallest possible quantity of water, even what is called moisture, may, as a series of individual liquid atoms, be vaporized and dissipated by proportional equivalents of heat. We are thus led to recognize the conversion of *single liquid atoms into vapor atoms*, as the only means by which each could obtain that degree of buoyancy which would enable it to rise and pass into the atmosphere.

What then takes place in vaporizing a single atom, must be equally applicable in the case of the myriads constituting a body, or even a single drop of water, and which presents the type, principal, and process of the change as truly as if it were one of a body at the boiling point.

With reference to the distinction between *latent* and *sensible* heat, it is manifest that the first equivalent, or unit, of heat absorbed by each single atom of *ice*, and by which it is converted into a *liquid* atom, must be *latent*, and be rightly termed the *statical* heat, inasmuch as it produces the true *status of liquidity* without exercising any dynamic effect—the indicated temperature remaining the same, say at 32° Fahr., both of the *solid* and the *liquid* atoms. This complement of latent heat *being thus satisfied*, the next unit or increment received must, *ex necessitate*, be *sensible* or dynamic; and as such produce a further physical change in the constitution of the atom, as already explained, namely, converting it into one of vapor, and imparting to it the property of repulsion, with enlarged volume. Now, as we cannot conceive an atom of ice in a semi-solid, semi-fluid state, neither can we conceive an atom of water in a semi-fluid, semi-vaporous state. We have, then, no alternative but that of attributing to this last unit or equivalent of heat, the cause which changed its *negative* character of *attraction* to that of a *positive* or *repelling* action. When, also, we take into consideration the inappreciable minuteness of each atom of the liquid, we have sufficient grounds for not undervaluing the effect of heat in its units on the single atoms of the liquid mass.

It is not necessary that we should here adopt, or even examine, the recognized theory of *latent* heat being in the proportion of 5 to 1 of sensible heat. Were that admitted, we should have to infer that each liquid atom, on be-

coming one of vapor, had been combined with 5 units of heat in the latent state, and one in that of sensible or thermometric. (How far it may be probable that *two* units are necessary to form the complement of *latent* heat, ariseing from the known facts that each atom of the liquid is a compound of two atoms—hydrogen and oxygen—in a wòrd, that we have to deal with an *electrolyte*, must be determined hereafter by others.)

The requisite equivalents of latent and sensible heat being absorbed by, associated with, or united to the liquid atom, its conversion into one of vapor *is complete*, and from the state of an *inelastic* body, with the property of *attraction and mobility*, it has assumed that of an *elastic* fluid, with the opposite properties of *repulsion and divergence.*

Looking then at the fact of each atom exercising an influence in proportion to its weight, volume, and power of repulsion, we are enabled to appreciate and apply Dalton's rule, that "*the force and pressure of steam is the same in equal weights and at all temperatures.*" In other words, that a grain weight of water, or a million of its atoms, when converted into vapor, will exercise the same "*force and pressure,*" whether raised from a body of water at 32°, or 212°.

By proceeding in this synthetical mode, we logically infer that the *temperature and pressure* must be in the ratio of *quantity, or number of atoms present*, in any given space —*each representing unity*—pressing, by the mere effect of accumulating numbers, on the bulb of the thermometer, and producing a commensurate effect in the indications of temperature. When, therefore, we say that vapor is at the temperature of 212°, it cannot be supposed that the temperature of each atom was 212°. Were that the case, the aggregate of heat imparted would, by accumulation, become inconceivable. We are then compelled to

consider the indicated temperature merely as *the sum of the units then present.*

By this simple process, we shall find that the alleged anomalies, hereafter to be noticed, will be disposed of, while the *number or quantity present in each given space,* will be the measure of *elasticity, pressure, force, volume, and temperature,* and the several other conditions incident to steam in the mass.

In illustration of the theory of unity, or units of heat being sufficient for the generation of vapor, let us suppose a sheet of paper containing the smallest possible quantity of water, say in the state called *damp.* That quantity must still, however, be regarded as an attenuated sheet of liquid atoms. If the paper be then exposed to the air, at a temperature but a single degree in advance, these atoms will nevertheless be subject individually to the same process, and receive their respective equivalents of heat, and be converted into atoms of vapor, as if they had been portions of a large mass of water. Each, then, will also become as true and complete an atom of vapor as if it had been generated at the boiling point.

What, then, may be asked, is the quantity of heat which a body of water could receive and absorb, while still retaining its liquid form? Can it be more than that which its atoms had respectively received on being changed from the fixed state as *ice* to that of *liquidity?* If so we are not warranted in assuming that the water, or *unvaporized portion* of the mass, can have any free or sensible heat: in a word—that it cannot be thermometrically heated or influenced so long as it retains its *status as a liquid.* Why, then, look to it for that *sensible* heat which influenced the thermometer at 212°, or any other figure, seeing that the vapor, continuously formed and retained in the water, sup-

plies an ample and legitimate source of influence, and *that* this view is at the same time consistent with physical appearances.

The oversight which so universally prevails consists in looking in the wrong direction for the results of the heat applied: 1st, in assuming it to be *chemically* combined with the body of the water, instead of with those only of its elementary atoms with which it comes into contact; and 2ndly, forgetting Dalton's well-established law, that vapors, as *elastic fluids*, are but *mechanically mixed* with the liquid medium in which they may happen to be.

In neglect of these two all-important considerations, we look to the body of the water, *in its liquid form*, as the chemical recipient of the heat, in the very face of the vapor which we see mechanically disconnecting itself from the water, and thus carrying away that heat.

Water, then, in the state of liquid, and at all temperatures, must be considered as a *mechanical compound* of liquid and vapor particles. So also the boiling point, and the temperature of 212°, must be considered without reference to the water, but rather as irrespective of its presence, as if it had merely represented a vacuum.

We will now continue the inquiry as to what takes place in arriving at this temperature of 212° and after it has been reached, and in what the process of ebullition consists.

SECTION VI.

ON EBULLITION.

This section is perhaps the most important of the series, and brings most prominently into view the discrepancies between the prevailing theories and those physical illustrations which experiment places within the scope of our senses. By these illustrations we are enabled to understand and appreciate the cause of those oversights which have so long led us astray as to the relation which heat bears to fluids and vapor. They are, however, so palpable, that it the more excites surprise how long and perseveringly we have wandered from the right road into the field of conjecture, and almost fiction, while the landmarks, which nature has unmistakably supplied, were so visible, but so neglected. To these, then, as marking the various and progressive stages of nature's processes, attention is here invited.

When, for instance, we see globules in a body of water, or bubbles breaking on the surface, we are warned not to pass them by as trifles unworthy our attention. On the contrary, we should inquire both into their causes, and the results to which they lead. To neglect them in chemical researches would be as unwise as for the mariner to disregard the lights and beacons which have been set up to direct him in his otherwise pathless course.

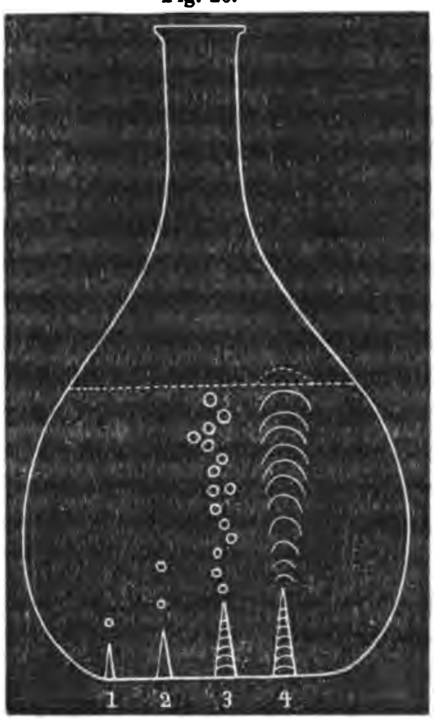
In the inquiry before us, we should watch and examine whatever comes within the reach of our outward senses,

and avail ourselves of their aid in tracing nature's various and apparently mysterious workings.

Having already described the phenomena accompanying vaporization, or the conversion of single atoms of a liquid into those of vapor, and the peculiar repellent properties which the atoms possess after such conversion, we have next to consider the effect of a continued application of heat to water.

Ebullition has been so variously described by writers that we may be permitted to doubt their accuracy. Some of the prevailing errors, however, may here be noticed. Among these is the very ordinary one of connecting the mere appearance of *surface bubbling* with the fact of *boil-*

Fig. 20.



ing—associated as the latter is with the high temperature of 212° Fahr. To avoid this (which will hereafter be shown to be a palpable error), we must examine the process, in all its details, and with a degree of attention it has not hitherto received.

With the exception of the air bubbles and the cloud or flame-like appearances already described, which accompany the continued application of heat, nothing will be visible until the thermometer indicates a temperature of about 180°. Small luminous spicula will then be seen starting from the bottom, in conical flashes; their luminosity arising from the light of the flame beneath. If the process be carried on gently, and in sufficiently tall vessels, each of these cones will be seen producing a single small globule as shown at No. 1, Fig. 20, which globule may be distinctly traced in its ascent to the surface. It is a mistake, then, to suppose that visible globules, once formed, are condensed, or disappear before they leave the water; this only occurs when they burst, and are dispersed in the air at the surface.

As the generation of vapor in the water proceeds, these spicula increase in number and size, each giving out two or more globules as at Nos. 2 and 3. Subsequently, these are produced in such rapid succession, and from the base of the same spiculum, as to give the cones a vibratory, or rather pulsatory character, each pulsation resulting in a separate globule, and containing, probably, myriads of atoms.*

* Gmelin observes :—"Atoms are not infinitely small, in the mathematical sense of the word, but bodies of determinate magnitude, which cannot be separated into smaller parts, either by mechanical or other forces. They are of definite weight, definite form; and these are constant in the atoms of the same substance, but may differ in those of different substances."

In such number are these globules formed, that the body of the water becomes so full of them as to resemble the white flakes of a shower of snow. This separation into globules will continue until the temperature is near 212°, when each succeeding cone or spiculum, with its rapid pulsations, will ascend in an unbroken series—producing on the surface, by the rapidity and mechanical force of their ascent, the appearance called ebullition, or boiling, as shown at No. 4.

It is from these spicula that the sounds proceed, which have been described in such a variety of ways. Each spiculum being as it were *a hollow cone*, on its rise and separation from the bottom, the water which it had displaced returns with a sharp action or blow, striking the vessel, and producing a sound, the character of which will be determined by the nature of the vessel, tin or copper giving a shriller note than glass.*

These cones are in fact, *incipient ebullition*, or the first stages of those *groupings of vapor* which ultimately form large bubbles on the surface, and where, on reaching the lighter medium of the air, they enlarge to the full volume of which they are susceptible under atmospheric pressure.

It is here important to observe that although the body of the water may appear full of these small globules, they nevertheless originate, or are derived from a very few cones or spicula, the number of which diminishes even after ebullition has begun. To obtain a better class of observations, it is advisable to use large beakers or bottles, containing 5 or 6 lbs. of water, and to apply the heat

* If distilled and filtered water be raised to the boiling point in a large glass bottle, but half-filled, and without being corked or closed, each separate sound arising from each spiculum will then be the more distinctly heard.

gradually—thus allowing time for noting the origin and ascent of the globules, and affording a longer interval for observation between their successive formations.

As the invisible atoms of vapor generated from the bottom stratum will necessarily be more numerous than those in the strata above it, among which they may become diffused, and farther asunder, the water at the bottom will sooner arrive at what may be called *local saturation.* When that takes place, there arises a remarkable tendency in these vapor atoms to *rush towards any mote*, point, or foreign matter that may be present, and by which these cones or aggregates of vapor are formed.

If the glass be perfectly clean, and the water free from foreign matter, none of these aggregates or globules will appear, and the process, even beyond the boiling point, will proceed without such groupings, and consequently without noise or ebullition.

As the same weight of water will be evaporized by the same amount of heat, ebullition does not appear to have any effect in increasing the quantity or rapidity of vaporization; this will, however, require a course of more accurate experiments, as being contrary to the received opinion. Dr. Ure distinctly says that ebullition favors vaporization. This, therefore, as a moot question, seems open to further inquiry, and will no doubt be determined by future experimenters.

The atoms of vapor, which, by rushing together, form these luminous cones, with their resultant globules, will not be allied by forces acting in the way of *cohesion*, but are mere mechanical, probably electrical aggregates. Their aggregation, however, being apparently so opposed to the mutually repellent action which characterizes the atoms of elastic fluids, we are forced to infer that some new

and different influence is at work in producing this effect. Being so direct and invariable an accompaniment of the addition of heat (*when foreign matter is present*), it seems a legitimate inference to consider it as *thermo-electric*—the more so as this tendency to rush towards points bears so strong an analogy to the tendency which results in the aggregation and discharge of vapor from a cloud in a *positively* electrical state, when any point in an opposite or *negative* state is brought within the range of electrical influence.

These groupings are, at first, formed only among the atoms of vapor, which emanate from the lowest stratum of the liquid mass. As soon, however, as the quantity of vapor in the water increases, and the mass becomes homogeneous, and saturated with it, this tendency to rush towards foreign matter will be found to prevail *throughout the body of the water*, even to the surface stratum. Instances of this will presently be given.

Another feature of these groupings is, that they are not confined to any particular point, but change their position as the points or foreign matter move about on the bottom of the vessel, as presently will be shown.

Where, however, any mote or point may appear to be *fixed*, or adhering to any locality, it will remain there as a *permanent nucleus* for such groupings until it be removed.

The most important peculiarity of these groupings is, that they have no relation to *the heat absorbed at their respective bases.* They are merely composed of the vapor atoms which have been already generated in their vicinity, and within the range of some certain influence, whether that be electrical or otherwise.

With reference to these appearances, Sir Robert Kane observes:—"Water boils in a glass or porcelain vessel under a pressure of 30 inches (ordinary atmospheric pres-

sure), not at 212°, but 214°: and in graduating a thermometer it is necessary to use metallic vessels. This latter appears to *favor ebullition* by the minute irregularities of its surface affording a nucleus for *steam to form*, as a crystal dropped into a saline solution facilitates the crystallization; and if the smooth surface of the glass be removed in a single point by a scratch with a diamond, the bubbles of steam will be seen to *form there* before *the general mass of liquid* begins to boil."

In this description there are some points which will not bear the test of examination. In the first place, the "*general mass*" of a liquid does not boil. Ebullition, or boiling, is solely the local commotion which originates in the several groupings formed at the bottom (as hereafter explained), and then rising through the body of the water.

To say, then, that "the irregularities afford nuclei for the "*steam to form*," is confounding the *visible appearance* of steam, in the shape of globules, with its invisible *generation*. Steam can only be formed or generated by the union or further increments of heat with additional particles of the liquid, and can, therefore, have no relation to the roughness caused by the scratch of the diamond. The bubbles, above spoken of, are the mere aggregates of the vapor atoms *previously formed*, although invisible, inasmuch as they must exist before they can so rush to the points or projections produced by the rough edges of the diamond scratch, or any other projections or motes accidentally presented to them. So the vapor atoms in the air must have previously existed before they could be grouped by the electric action, and descend in a shower of rain drops, as seen after a thunder storm.

But the analogy between *crystallization* and the *formation of steam globules* is inadmissible. The former is the

mere collocation, or arranging, under certain laws, of solid atoms *already existing* and without reference to any accession of heat. The crystal dropped in then merely facilitates the arrangement of the saline or other particles already existing. Vapor atoms, on the other hand, do not exist until they have received their respective equivalents of heat.

Metallic vessels differ from glass or porcelain, in reference to ebullition, merely in having rougher surfaces, and, consequently, presenting more projections to act as nuclei, or negative points, for the formation of these aggregates or vapor atoms.

It may here be observed that when impure, undistilled water is boiled in a glass or metallic vessel, the act of vaporization leaves behind and attached to the vessel the matter that may be *in solution*. These deposits then become fixed points or projections, and form nuclei for the groupings; and as metallic vessels cannot be cleaned as glass may, permanent sources or series of nuclei are thus formed. (Occasionally small groupings may be noticed on the glass of the thermometer bulb; these should at once be removed by withdrawing and wiping it with a dry chamois leather.)

Professor Brande, referring to these appearances, observes:—"The influence of the *quality of the surface* of the vessel, its cleanliness, and other circumstances, upon the boiling point of water, has been examined by F. Marcet. (Ann Ch. et Ph.) He found that in glass flasks the boiling point of water varied with *the quality* of the glass, fluctuating between 100° and 102° of the centigrade scale. If the glass vessel be *perfectly cleaned* by solution of potassa, or by sulphuric acid, and all *chemical and mechanical* matter removed, water may be raised in it, *without boiling*, to 105°

(=220° Fahr.) In all these cases the degree of *adhesion between the water and its containing vessel* appears to be the cause of the fluctuation at which boiling ensues."

Here we have the same theory of the *water being the recipient of the heat*, but without reference to the existence of the vapor; yet to that vapor and the increased quantity present is the increase of temperature alone attributable, irrespective of ebullition or boiling.

That the *adhesion* between the water and the containing vessel can have no influence whatever, will be evident, since ebullition and violent commotion (which is merely accidental) may be produced, even when the glass is perfectly clean, and the water absolutely free from all matter in solution.

None of these writers, we see, refer to the true and first cause or source of ebullition, namely, the *presence of vapor in the water in excess of saturation;* yet, without this, no groupings, and, consequently, no ebullition can take place, as such does not begin until the quantity of vapor present approaches the point of saturation. This point, as already stated, will be reached when the *diverging*, or *self-repellent* force of the vapor atoms, *inter se*, and throughout the mass, are in equilibrio with the *converging pressure* arising from the density of the water medium surrounding them.

Now, this equilibrium, of itself, demonstrates the presence and influence of the vapor; since, if the heat had been absorbed by the water (*qua* water, or liquid atoms), these atoms having no repellent property, and being influenced by gravity alone, would rise and remain uppermost; consequently, uniformity of temperature throughout the mass could not exist.

This point of saturation, commonly called the boiling point, will then be determined solely by the quantity of

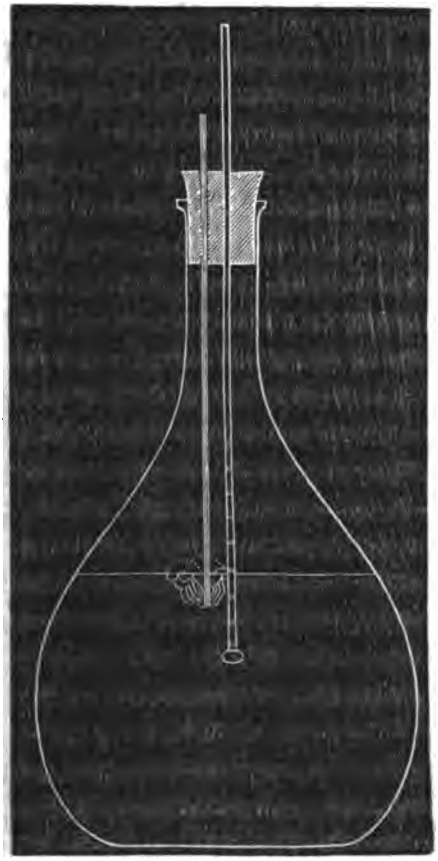
vapor present in any given space; in water it takes place when that quantity indicates the temperature of 212° Fahr., in alcohol 176°, in sulphuric acid 630°, and in mercury 660°.

Let it now be assumed that 1,000 atoms of vapor, in any given space, is the saturating quantity in water; until that quantity be present there will be no tendency towards these groupings, and, consequently, there will be no ebullition; nor, even though the required quantity should be present, *unless there be some motes, points, or foreign matter* present. To this alone is attributable the fact of the temperature of 220° and upwards being reached without ebullition, bubbling, or any disturbance or noise.

Some experiments may here be described as affording demonstrative evidence in support of these statements, viz.: —Five pounds of well-distilled water at 60° were put into a perfectly clean bottle, with a thermometer inserted in a cork, as in Fig. 21. A small hole, one quarter inch wide, was left in the cork, to allow the air and vapor to escape, and prevent any extra pressure. Through this hole also any foreign matter may be introduced. An Argand burner being placed under the bottle, and the temperature raised to 212°, or above it, and *without ebullition*, the water may then be said to be *saturated with vapor*. In this case the water, according to Dalton's law, will be merely acting the part of a vacuum for the vapor or water gas.

To test the presence of the vapor, and the fact of saturation, let the end of a fine rod, or tip of a feather, be introduced through the hole in the cork, as shown in Fig. 21, and pushed down *just below the surface of the water.* The presence of an excess of vapor will then be ascertained by its rushing into contact with the object introduced, around which groupings of vapor atoms will be formed, and collect in bubbles on the surface of even a large size.

Fig. 21.



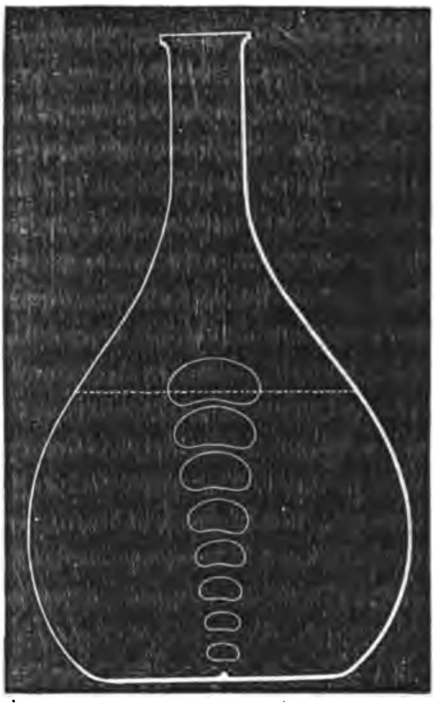
On pushing the rod or feather down into the water, and even to the bottom, these groupings will be increased, in number and size, as long as there remains any vapor in excess of saturation. So soon, however, as that excess is discharged, the temperature will have fallen to the saturating point, or 212°.

When it is desired to have the vapor collected in the water, without ebullition, it is only necessary, as Professor Brande observes, that the glass be perfectly cleaned, and

all matter in suspension removed from the water.* This state of things affords a favorable opportunity for observing the vapor rising in the cloud or flame-like form through the water, even after the thermometer indicates a temperature far above 212°.

As it will be impossible to have the water absolutely free from motes or sedimentary foreign matter, when any such

Fig. 22.



appear in forming globules, and interrupting the tranquil state of the water, let the heat be reduced to what will

* For this purpose the water while hot should be filtered several times through a few folds of fine cambric. This is preferable to cotton, the fine fibres of which pass into the water.

merely keep the thermometer at 212°. The whole will then remain undisturbed, except at short intervals, when, by reason of some still unremoved and invisible motes coming into contact with the bottom of the vessel, groupings and globules will then occasionally arise, and in their ascent so accumulate the vapor as to produce large aggregates on the surface, as shown at Fig. 22. One of these aggregates is there represented rising and enlarging, by accumulation, as it ascends to the surface, and suddenly forcing, through the hole in the cork, the volume of vapor which had been thus collected and suddenly discharged.

We have here the direct cause of what is called *bumping* and *explosions.*

The effect of these intermittent results is so connected with the ordinary process of ebullition that it will be interesting to give the following extract from "*Faraday's Chemical Manipulations*," descriptive of these results, although it leaves unexplained their true cause.

"The evolution of vapor is in many cases very much facilitated by the *addition of substances* having apparently no chemical action; and the process of distillation is not only thus facilitated, but rendered possible and easy in cases where otherwise it would be almost unattainable. If diluted alcohol, spirits of wine, or certain alcoholic solutions be distilled in glass vessels, the vapor is frequently evolved with difficulty; the contents of the retort *at one moment not boiling at all*, and at *another bursting throughout into a mass of vapor and fluid, which fills the whole body of the vessel.* This endangers the sudden expulsion of part of the substance, causing serious derangement of the process, and is also accompanied with such agitation of the fluid—such bumping and shaking of the retort—as at times actually to endanger the safety of the

whole; *for when the vapor is formed*, it is with such force as to produce a dull *explosion.* This is prevented by the introduction of a *few angular or fragmented pieces of solid matter* into the retort, of such nature as not to be acted upon by any of the substances present. For this purpose, *metallic substances* are best; a piece of *platina foil* cut by scissors into narrow slips, so as to resemble a fringe, or seven or eight inches of silver, platina, or copper wire, pressed up loosely, or platina and silver filings, are then very useful; so, also, is a fragment of cork or a piece of torn cartridge paper, any of which will generally cause the regular and tranquil evolution of vapor, and occasion the distillation to proceed quietly and satisfactorily."

It will here be seen that the heat is assumed to be absorbed by the *liquid*, as evidenced by the supposed "bursting throughout into a mass of vapor and fluid." So, again, when it is said, "when the *vapor is formed*, it is with such force as to produce a dull explosion." It is only necessary to say that, there is no such thing in nature as this *sudden formation of vapor in a mass;* besides, the *gradual* formation of vapor, and its *accumulation in the water* fully explains and satisfies these apparent anomalies. Again—

"The student should be cautioned against the sudden introduction of these promoters of vaporization, whilst the fluids are hot. If upon the occurence of bumping during a distillation of alcohol, sulphuric acid, or any other fluid, in glass vessels, a piece of any of the substances mentioned were suddenly introduced by the tubulature, it is probable that the consequent burst of vapor would be so instantaneous and strong as to do more harm than the bumping itself.

"The safer method is to remove the source of heat for a moment; then, opening the tubulature to introduce a

platina wire, *letting it touch only the surface of the fluid at first*, and introducing more of it as the ebullition occasioned by it ceases; when that is over, the wire should be withdrawn, the cork, the platina, or whatever, according to the nature of the fluid within, has been selected, be introduced, the stopper closed, heat applied, and the distillation proceeded with."

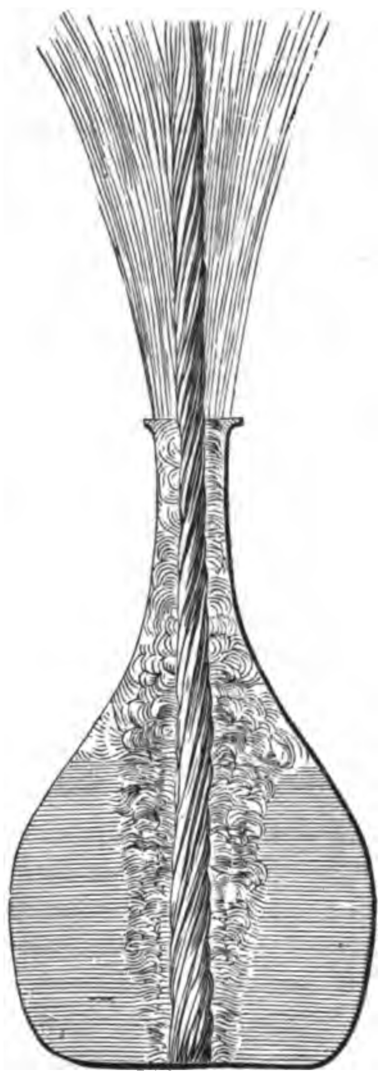
It may here be observed that the cause and effect thus described by Professor Faraday arise solely from the presence of vapor *in excess* of saturation, and its sudden grouping and discharge by the introduction of foreign matter, as already described.

The idea, then, of the contents of a vessel *bursting throughout into a mass of vapor* is simply a mistake. The vapor must have been *previously* formed and present in the vessel, before it could thus suddenly produce these dangerous aggregates.

The very caution here enjoined, on introducing the platinum wire to "let it touch *only the surface* of the fluid at first," shows that the groupings and presence of the vapor had not been anticipated. The touching *the surface* has merely the effect of enabling the vapor then in excess to escape gradually. Had the platinum wire been dropped in and allowed to fall to the bottom, a greater and more rapid discharge would have been the result. All this is fully illustrated at page 112, Fig. 22, and the case described by the Professor may be as well, and correctly exemplified in water, as if the liquid had been any of those he alludes to. As the rise in the thermometer above 212° indicates the presence of vapor in excess of saturation, this may be physically demonstrated by the discharge of such excess either gradually or suddenly. The former mode has been already explained, the latter may be effected by removing

the cork, and rapidly plunging to the bottom a piece of wood, brick, or other large and rough body; the excess of vapor will be discharged with considerable violence, as shown at Fig. 23.

Fig. 23.



If the brick be attached to a thread it may be drawn out, and the process be repeated *ad libitum.* The wood, brick, or other body should be free from loose matter, otherwise some particles may be left behind in the vessel, and defeat the successful continuance of the experiment. In this case let the water be refiltered—it is but a two minutes' operation—and the process may be repeated.

It is remarkable how much consideration is given to the description of matter which is supposed to produce these sudden effects, viz., metallic filings, platinum in strips, etc., while no inquiry is made as to their cause. Now, any body that will fall to the bottom, or that can be held there, will

answer equally well. Pieces of coal, brick, iron, or wood will equally serve as nuclei for the groupings of the vapor.

As *points* are peculiarly favorable, a large goose-quill feather, from the numerous points it presents, answers the purpose well.

By these experiments we ascertain:

1st. The existence of the vapor in the water.

2d. That the excess of such vapor beyond the saturating point may be discharged gradually or suddenly.

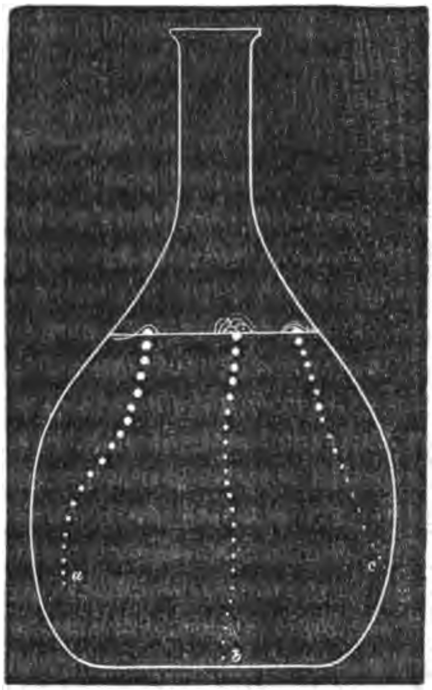
3d. That ebullition is the mere result (mechanical or electric) of the tendency of the vapor to rush into contact with any foreign matter that may be present and furnishing points or nulcei for aggregation.

It may here be observed, that instead of a piece of wood or other heat conductor, if we introduce a glass rod, no such groupings or discharge of vapor will take place. Whether this is caused by the glass being a non-conductor of electricity, or by the mere absence of points or roughness, must be determined by future investigators.

On this tendency to rush to foreign matter, Professor Brande observes: "In a glass flask the boiling point is proportionally high, and irregular, but upon throwing in a few metallic filings and insoluble materials, *the generation of steam* is facilitated, and the boiling point falls to its standard." Here again the mere *aggregation* of the steam *already existing*, is mistaken for its *generation*. On what principle, it may be asked, could the introduction of *cold metallic filings*, or other bodies cause the *generation* of steam. Besides, it seems the reverse of fact that facilitating the *generation of steam* should have the effect, not of increasing, but of reducing the temperature, although *its escape* would have that effect. Other proofs may here be given of the tendency of vapor, in excess of saturation, to

rush towards foreign matter. Small and scarcely visible motes will frequently be *found floating through the mass* of the water. These act the part of points, and become nuclei towards which the vapor will rush, so soon as the saturating quantity shall be present, *but not a moment sooner.* These floating objects will then cause the appearance of a continuous stream of aggregates or small globules, as seen at *a*, Fig. 24. So, also, when any motes adhere to

Fig. 24.

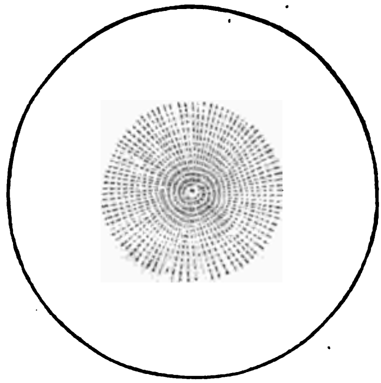


the bottom or sides of the vessel, as at *b* and *c*. These, however, may generally be removed by the end of a wooden rod or wire, and the effect will cease.

Looking down on these groupings, on the bottom of the

vessel, and supposing they were visible, their formation may be thus described; see Fig. 25. The centre here represents on an enlarged scale, the mote or object which acts as the nucleus; the dotted radii representing the vapor atoms rushing towards it. In section they may be represented as in Fig. 26.

Fig. 25.



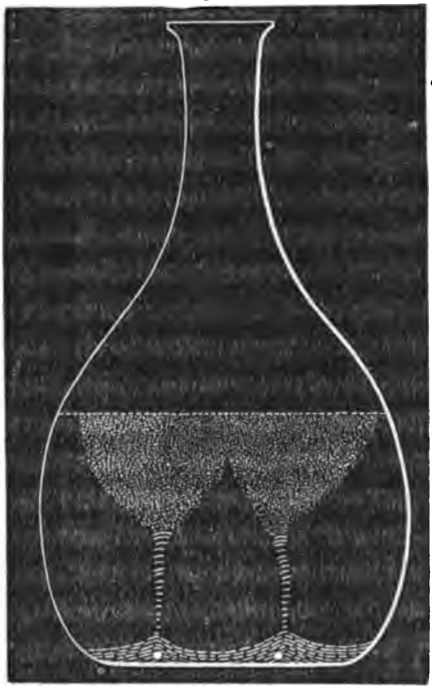
That these illuminated cones or aggregates are in no way connected with the vapor formed at their respective bases may thus be proved. If a rod be introduced into well-filtered water, and made to touch the bottom, the congregating of the atoms will follow it, wherever it may be placed. By moving it across or round the bottom several interesting and convincing proofs will be afforded in illustration of this tendency to group, thus making the source of ebullition to pass at will to or from any part of the vessel. Now, as we cannot associate the generation of vapor with the mere spot on which the rod may rest, while the remainder of the bottom appears insensible to the influence of heat from beneath, we have no alternative but the conclusion that the rod or other body introduced has merely acted the part of a nucleus of attraction for the vapor generated over the *entire bottom surface of the vessel.*

It is thus shown that ebullition is merely *accidental*, and has no reference to the *generation* of the vapor, and that it is solely the result of the aggregations of myriads of

atoms, *previously* formed, and irrespective of the heat transmitted and absorbed in any particular locality.

This will be further shown if we attach *fixedly* to the bottom of a glass vessel any number of small bodies which will answer the purpose of permanent nuclei. For example, describe on the bottom of the vessel any figure, as a circle or cross, with thick oil paint, on which some pounded brick, or coarse sand, or bird-seed may be dropped. On the paint becoming dry these bodies will remain permanently fixed on it. Heat being then applied, and the temperature raised to 212°, the process of accumulation of the vapor will appear confined to such figures, the remaining portion of the bottom surface being *apparently* ineffective in the production of vapor or its globules. Figs. 27 and 28 represent such cross and circle, the dotted lines in Fig. 27 indicating the direction of the rushing vapor into contact with such nuclei.

Fig. 26.

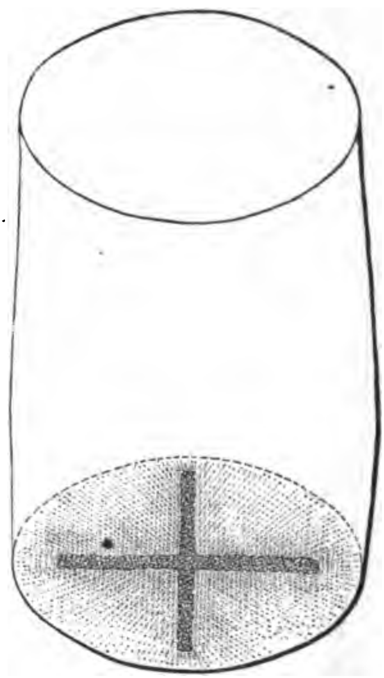


From these illustrations we learn how liable we are to be led astray and form incorrect inferences when experiments are made in metallic or opaque vessels, or without

due attention to the minute details which a closer examination would have discovered.

It is here worthy of remark, that these movements, as regards the physical or electrical characteristics of ebullition, have hitherto been very inadequately examined or recorded. Among the most remarkable of these (and which is always visible when glass vessels are used), is that, whatever may be the violence or extent of surface ebullition or bubbling, the whole will proceed exclusively from a very few, often but one or two points on the bottom of the vessel, and where the groupings are formed. We know that when heat is applied to the bottom of a vessel it acts against the entire of the *outside* surface, and that the liquid atoms are in contact with every part of the *inside* surface. Nevertheless, we see that the formation of the bubbles or aggregates which produce surface ebullition is confined exclusively to the spots where motes or foreign matter are presented to them.

Fig. 27.



In further illustration of the rapid formation of these groupings, and the violence of their action, the following may be mentioned: Into a beaker, containing about one pound of water, throw as much pounded brick or coal as will cover the bottom about an inch deep. Apply the

heat of an Argand burner beneath, and let the temperature be gently raised to the boiling point. As soon as the groupings begin to be formed they will have the effect of forcing up the brick or coal with the appearance of miniature explosions, as shown in Fig. 29.*

Having described the process of ebullition with reference to those appearances which are too palpable to be mistaken, although hitherto either unnoticed or attributed to causes which will not bear the test of examination, it may be well to give some few illustrations from high authorities of the prevailing theories, for the purpose of comparison.

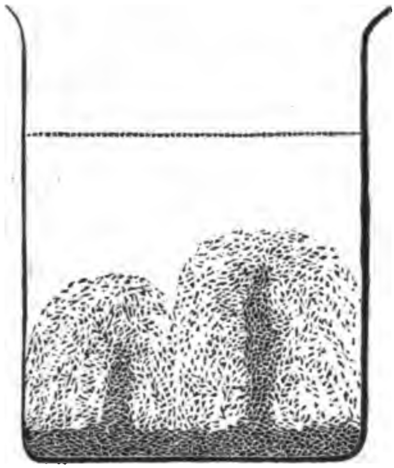
Fig. 28.

THEORY OF PROFESSOR MILLER.† "The process of ebullition may be beautifully shown in a common flask, heated from below. At first, bubbles are formed at the bottom of the vessel; these bubbles are *condensed and disappear* with a peculiar vibratory sound before they reach the surface."

* The coal or brick should be broken about the size of unground pepper, the fine dust particles being sifted out, and the whole well washed in boiling water. Care must be taken not to confound the ascent of particles of the coal which at first will be carried up by minute globules of air, which attach themselves to them and ascend with the vapor.

† "Elements of Chemistry," by W. A. Miller, M.D., F.R.S., F.C.S., Professor of Chemistry, King's College, London.

Fig. 29.



The vibratory sound is here assumed to take place in their passage upwards to the surface. This is unquestionably an oversight, as already explained.

"*At length* the temperature of the whole mass, if fluid, becomes uniform." It has already been shown that the uniformity of temperature, until ebullition has commenced, is owing to the *diffusion* of the vapor, and that it takes place not, as above, *at length*," but within *the first minute* after the heat has been applied, and continues with unmistakable uniformity up to the temperature of saturation. It is passing strange that this uniformity of temperature should so perseveringly be attributed to some occult cause, while the movement in the water, as shown at Fig. 3, so demonstratively illustrates that process of mixing and diffusion, which produces uniformity throughout.

Again—" The bubbles of steam, as they form, rise to the surface and break, emitting a peculiar transparent invisible vapor, designated as steam, when its temperature has been

sufficiently reduced by the external air to bring it back to the liquid form in exceedingly minute globules."

It is here erroneously assumed that *the whole* of this emitted vapor becomes cooled by the external air, and reduced to the *liquid form*. The visibility of the white cloud of globules in the air, however, is the result of *portions only* of the vapor atoms being cooled down to the liquid state, and which, as in the case of soap bubbles, form a liquid film or envelope, enclosing the pure vapor. This will be further illustrated hereafter.

Further—"*Adhesion* of the fluid to the *surface of the vessel* that contains it has a marked effect in raising the boiling point, in consequence of which water sometimes boils at 214°, if a pinch of metallic filings be dropped in."

This adhesion to the sides has been already disproved, as having no influence whatever in raising the boiling point. So the metallic filings have been shown to have no *direct* effect, and merely afford negative nuclei for the aggregation of the vapor atoms and their consequent discharge.

"By long boiling of the water, the air becomes nearly expelled; in such case, the temperature has been observed to rise even as high as 360° Fahr. in an open vessel, which was then shattered with a loud report by a sudden explosive burst of vapor."

(There seems likely to be here some error, either as to the high indicated temperature, or the vessel being an open one.)

No attempt is here made to explain the cause of this extraordinary and sudden burst of vapor, or how it could have been produced where no additional heat was supplied, and where nothing was supposed to have previously existed but water.

To reconcile this anomaly of mere water, in its liquid

form, and in an open vessel, producing a sudden explosive effect, the following ingenious but altogether unsatisfactory theory is given, viz.:

"In this case the force of *cohesion* retains the particles of the liquid throughout the mass *in contact with each other* in a species of *tottering equilibrium*, and when this equilibrium is overturned at any one point, the repulsive power of the excess of heat *stored up in the mass* suddenly exerts itself, and the explosion is the result of the instantaneous dispersion of the liquid."

It would be of no practical value to analyze the many points in this ingenious, but altogether supposititious case. It may, however, be remarked, that we have here a sudden and even powerful effect produced without any sufficient or apparent cause. Besides, when it is said that the "repulsive power *stored up in the mass*, suddenly exerts itself," it may be asked where was this *store of heat*, and with what was it combined? By what marvellous agency was it available for this sudden effect so wholly irreconcilable with all the known laws of heat? We have here also the anomaly of considering the heat as being *stored by itself*, and unassociated with matter of any kind, until, by its own force, it produced an instantaneous dispersion of the liquid.

The Professor thus continues:—"The *gradual absorption* of heat in the passage from the liquid to the gaseous state" (manifestly considering the liquid as a *body*, apart from its elementary particles), "is not less essential than the corresponding absorption in the passage from the solid to the liquid condition. Where it otherwise, every attempt to boil a saucepan or a flask of water, or other liquid, would be attended with explosions, from the *sudden formation, the moment the boiling point was attained.*"

Now this "*gradual* absorption of heat" would imply an

equally *gradual* change in the character of the atoms absorbing it, and thus negative any *sudden* formation of vapor. Here, also, it is assumed that the absorption of heat by the water continues until the boiling point is reached, thus altogether ignoring the consecutive formation and escape of the vapor from the moment the heat is applied, and which effectually prevents the possibility of any *storing* it up. Altogether, this theory appears irreconcilable with what we see in the ordinary experiment of boiling water.

We will now examine the theory of another unquestionably high authority.

THEORY OF PROFESSOR RANKINE.—"When the communication of heat to a liquid mass, and the removal of the vapor, are carried on continuously, so that the pressure throughout the mass of liquid is not greater than that of saturation for its temperature, evaporation takes place, not merely from the *exposed surface* of the liquid, but also from *its interior;* it gives out bubbles of vapor and is said *to boil.*"

As, however, evaporation, meaning the escape of vapor, can *alone* take place from the *exposed surface*, it is manifestly an error to say it can take place from its *interior.* Again,

"A resistance to ebullition is also offered by a vessel of a material which *attracts the liquid*, as when boiled in a glass vessel, and the boiling takes place *by starts.*"

It is somewhat fatiguing to have to repeat the refutation of this supposed relation of the adhesion of the liquid to the vessel. Attention to the process of boiling in a glass vessel will satisfy us that ebullition has its origin solely *at the bottom*, or where foreign matter may happen to be, and has no reference to the feeble attraction which liquids possess to the vessel employed, of whatever description it may be.

Another justly esteemed authority seems to have fallen

into the same path of error, as to the temperature of water apart from that of the vapor it contains.

THEORY OF PROFESSOR GMELIN.—Of the boiling point Gmelin observes:—"Since elasticity increases with the temperature, there exists for each body a certain temperature at which the elasticity of its vapor is a balance for the pressure of the atmosphere, and consequently cannot be restrained by that pressure. This *temperature is the boiling point* of the body. *At this point it passes into vapor*, in spite of the atmospheric pressure, as soon as the additional heat required to volatilize it is supplied."

It is not clear in what the temperature or elasticity here exists, as we are told it is only when the water has reached the boiling point that "*it passes into vapor.*" Throughout, also, the water and the vapor are spoken of as if they were distinct *bodies*, no reference whatever being made to the constituent particles of either.

Now, instead of the "temperature," or the "boiling point," let us put the words *quantity of vapor*, and the whole becomes intelligible. It is an error then to speak of a body of a liquid at the boiling point "*passing into vapor.*" The body or mass never can assume such a form. Gaseous or vaporous formation must be *progressive*, and is limited to the quantity or number of liquid atoms receiving their equivalents of heat and their conversion into vapor.

The last authority here to be quoted is the more important, as it brings before us the views of several Continental celebrities repeatedly referred to by our own chemical writers, but all alike illustrative of the unsettled state of the question.

THEORY OF PROFESSOR DIXON.*—Under the head of "the influence of foreign bodies in solution or in contact, on the temperature of ebullition," Professor Dixon observes:—"This subject has been examined with particular care by M. Magnus and M. Donny, and we propose to lay before the student the views of those writers on the *process of ebullition.* M. Magnus remarked, what had been observed by previous physicists, that water which had been well boiled *does not* generally *pass into the form of a steam* in glass vessels until it has acquired *a temperature considerably above that due to the force of its vapor*, and that the formation of steam then takes place *suddenly and with great violence.* From this it follows that the force requisite for the disengagement of the steam is greater than its expansive force subsequently, and the difference of these forces M. Magnus refers to the *attraction of cohesion* existing *between the particles of the liquid*, which requires to be overcome at the *moment of formation* of the steam, in addition to those pressures which the vapor itself subsequently sustains."

To this it may be objected, 1st, that there is no expansion of water apart from the vapor it contains; 2ndly, that water does not pass "*suddenly and with great violence*" into the form of steam, but in the most progressive manner, *pari passu*, with the application of heat, and in the ratio of the surface exposed to its influence; 3rdly, that the "*attraction of cohesion*," either between the particles of the liquid or to the glass vessel, has no reference whatever to the formation of steam.

"We can understand," observes Professor Dixon, "how

* "Treatise on Heat," by the Rev. Robert V. Dixon, Fellow and Tutor, Trinity College, Dublin; and Erasmus Smith. Professor of Natural and Experimental Philosophy.

the presence of bodies held suspended in a liquid, or the sides of the containing vessel, if these have a less attraction for the particles of the fluid than the latter have for one another, will lower the boiling point." On the contrary, it is difficult, if not impossible, to understand how the very feeble attraction of the particles of a fluid either to *each other*, or to the *sustaining vessel*, can have any, even the remotest, connection with lowering the temperature.

"Accordingly," he adds, "saw-dust, or insoluble powders, diffused through the fluid mass and the sides of a metallic vessel, which, as is well known, are never completely moistened at all points by water, lower the temperature of ebullition to that of the vapor."

This last remark of *lowering the temperature of ebullition* (whatever that may mean) only shows how our ideas become mystified when we leave the region of physical demonstration. Again,

"But if the water is boiled in a glass vessel, especially if the sides of the latter are perfectly cleaned by heating sulphuric acid in it up to 150° C., and then rinsing it with distilled water, by which contact is rendered more perfect, and the cohesive force of the glass on the particles of water, stronger, the *boiling point will rise* to 105° or 106° (centigrade scale). Now, as M. Magnus remarks, the action of the sides of the vessel, and of solid bodies in general, may *diminish* but cannot *raise* the temperature of ebullition."

Here is a contradiction which only the more shows how little of certainty even the highest authorities have obtained, for, he adds, "if the force of attraction of such bodies for the fluid molecules were stronger than that of the latter *for one another*, the only effect would be that the ebullition would *commence at the centre of the liquid*,

and not at the sides. Accordingly, no liquid can assume a higher temperature than that at which the expansive force of the vapor suffices to overcome the pressure and cohesion of the liquid." There is here much ingenuity and labor uselessly bestowed in the vain effort to prove an influence which assuredly does not exist.

THEORY OF M. DONNY.—The Professor then adds:—M. Donny, however, has been led by his investigations into the force of cohesion of liquids to conclude that the boiling point of water, as defined above, that is, the temperature at which water, perfectly free from all foreign bodies, would *pass into a state of vapor throughout the mass*, is considerably higher than even the highest limit assigned by M. Magnus."

It is only necessary to repeat that no such high temperature of *the water*, apart from the combined vapor, can exist, and that there is no such thing, as already observed, as a *body of water passing into the state of vapor in the mass.*

With equal truth, indeed, might he have spoken of the temperature of a *block of ice on passing into the state of liquid in the mass.*

M. Donny is then represented as concluding that "the mutual force of cohesion of the particles of water is equal to a *pressure of about three atmospheres*, and in this strong cohesive force finds an explanation of the phenomenon called *soubresaut*, or jumping motion, sometimes observed when in the state of ebullition, as well as, probably, of those fatal explosions which occur so frequently in steam boilers, and whose origin continues to perplex engineers and physicists." (This jumping has already been referred to and explained.)

Here, strange and contradictory as it may appear, this strong *cohesive* force is described as being the direct cause of the *repulsive* force, and even of the explosions in boilers.

M. Donny continues:—"By the effect of boiling, liquids lose the greater part of *the air* which they held in solution, consequently, the molecular attraction commences to manifest itself in a sensible manner, and permits the liquid to attain a temperature considerably above its normal boiling point; this elevation of temperature determines the appearance of *new bubbles of air*, the liquid then *separates abruptly* with a *soubresaut*, ('unexpected and irregular motion, a kind of start,') a large quantity of vapor forms, and consequently a reduction of temperature ensues, which restores a momentary calm to the liquid. Presently, the same causes reproduce the same effects, and the phenomenon is renewed with increased violence."

Now, this *soubresaut*, instead of being the result of the separation of vapor, is, in fact, that of the reverse—namely, of its aggregation. The contradictions and mystifications which characterize the development of this new theory, only expose the labyrinth into which we fall when we stray from the straightforward road which experiment points out and illustrates.

The concluding paragraph is here worthy of a quotation, from its originality and its entire deviation from the processes of nature.

"From these facts (observes Professor Dixon), proving the strong cohesive attraction of liquid particles joined to the well known tendency of all fluids to assume the vaporous state at all temperatures, M. Donny concludes that the *superficial stratum* of liquids possesses a peculiar property in this respect, and has hence been led to form the following theory of *ebullition*, viz.:—The elevation of temperature of a liquid [but which does not exist] produces the formation of small *bubbles of air* in *the hottest portions of its mass*, and, consequently, on *the side* of the containing vessel nearest the

source of heat; each of these bubbles [of air] presents to the liquid molecules which surround it a surface which facilitates the vaporization of these molecules; and when the tension of the vapor becomes sufficient to counterbalance the pressure to which these bubbles are exposed, there is no further resistance to the development of the vapor, which then forms currents that traverse the liquid, and produce the phenomena of ebullition. I think, then (continues M. Donny), we are justified in concluding that ebullition is nothing but a kind of rapid evaporation, which takes place at these *internal surfaces* of *a liquid* that bound the bubbles of *aeriform fluid* which are formed through its mass.

In this ingenious theory, air and its bubbles form an important feature in the phenomena of ebullition. Each of these bubbles (not the atoms) is supposed to be surrounded by a film of *liquid molecules*, and to facilitate their evaporation. Were this true, ebullition could no longer be practicable after the water had been deprived of its air. The whole theory, in fact, is merely imaginative.

Professor Dixon then adds:—"We will conclude these remarks on the phenomenon of ebullition with the observation of M. Magnus, that there does not exist an older physical experiment, nor one more frequently repeated than that of boiling water; but, nevertheless, what occurs was not sufficiently known, and even now much remains unexplained."

In this opinion both these professors may be assured of a general assent. It is, indeed, remarkable how much has been written and what ingenuity has been displayed in endeavoring to account for the ordinary phenomena of ebullition, and even how this theory of the cohesive force existing among liquid particles has led so many astray.

The practical value of these ingenious devices might be well tested by the application of Dalton's sound maxim, namely, that "no conception can be clearly grasped by the intellect if it could not be visibly depicted or embodied to the external sense." A sounder or more practical guide for future theorists could not be given.

Let us now consider what may be the purpose and utility of the ebullition process, assured, as we must be, that there is some wise object and provision of nature to be effected by it. It has already been stated that ebullition does not appear to have any direct influence in promoting vaporization; what then, it may be asked, is its practical effect or value in nature's economy? Two important purposes, at least, may be inferred. 1st. The preventing a useless, if not dangerous accumulation of vapor in liquids of all kinds, under the influence of accumulated heat; and 2ndly. The producing that all-important movement, *circulation*—the element of equal distribution of the heat and vapor in all directions throughout the mass.

Of the *first*, as vapor cannot escape from a fluid except at *its surface* (which will be further explained when we speak of evaporation), there would necessarily be an ever-prevailing tendency to its *accumulation in the water*, were there no other means of effecting its discharge than would be due to the area of that surface, under the mere operation of diffusion.

Now this object is directly effected by the rapid collection of the vapor atoms in the groupings which are seen in ebullition. Immediately as each group is formed, and by reason of its bulk and levity, it rises to the surface in the shape of a globule, and with an accelerated force escaping into the air, the body of the liquid being thus relieved from its presence

Of the *second* purpose: When these groups and globules are produced, they rise with a force and physical momentum due to their enlarged volume and levity. These aggregates of the gaseous element of vapor may, in their effect upon circulation, be compared to that of a balloon, mechanically forced upwards, by the pressure from beneath, of the heavier particles of the air. We know that the gas with which the balloon is filled would, *if liberated*, be discharged into the air, each atom ascending with a force due alone to its own specific gravity, but which would necessarily be slow and comparatively ineffective. When, however, the myriads of atoms of gas are brought together, and confined within the balloon envelope, the levity of the whole gives it an ascensional force and rapidity which carries it to the higher regions of the atmosphere.

In the same way, then, each group or globule of vapor, formed at the bottom of a body of water, is productive of precisely similar results. The *secondary* and equally important result is that by which *circulation* is directly affected. As the balloon ascends, and on each step of its progress upwards, it would leave a vacuum *below* it (as a ship moving through the water would *behind* it), were it not that the space is at once filled with the succeeding portions of the air (or water), and a mechanical action is thus produced.

Circulation then is the result of the *quasi*-induced current, consequent on the movement of a body through air or water, and in proportion to the rapidity of that motion.

In this inquiry we are but investigating, by the aid of experiment and inductive reasoning, the truth and application of those laws which determine the progress and influence of heat on matter and its motions, assured that in nature every thing is perfect in itself, and all, *apta inter*

se. The inferences here drawn do not rest on any baseless hypothesis, but on a clear view of the constant and unerring laws of nature as far as they are presented to our view, or within the range of our reasoning power.

Looking, then, at this branch of the subject in a general point of view, the cardinal facts appear to be—

1st. That ebullition, or the formation of bubbles, is nothing but the sudden aggregation or grouping of myriads of atoms of vapor *already formed* and existing *in the liquid mass*, and rushing into contact with some motes or points of foreign matter, accidentally or intentionally presented to them.

2nd. That these aggregates are composed exclusively of such vapor atoms as are in excess of the saturating quantity.

3rd. That the quantity of vapor required for saturation of any liquid has a fixed relation on the one hand to its density, and on the other, to the repellent action which its constituent atoms individually exercise, whether the liquid medium be water, ether, alcohol, oil, or mercury.

4th. That ebullition has no relation to the quantity or number of liquid atoms converted into vapor atoms, from any given surface, but to the number of such atoms that may have been enabled to form such groupings or aggregates.

5th. That without those groupings or aggregates, the vapor atoms, as they are successively formed, would individually rise, though invisible (by virtue of their separate enlarged volumes and diminished specific gravity), to the surface, and from that surface into the air.

SECTION VII.

OF VAPOR IN WATER.

ATOMS of vapor cannot be distinguished either *in* or *out* of water by reason of their minuteness. We have, however, sufficient grounds for being convinced of their presence in both cases. So long as they remain apart from each other, with their several diverging properties, they must remain invisible. So soon, however, as they congregate and form *globules*, they then come within reach of our senses. This congregating may take place both *in* the body of water, and out of it. When *in* the water, they form visible globules, which, by reason of their greater levity, rise to the surface, there burst, and pass into the air above it.

When *out* of the water, in a similar way, and when they come into contact with, and are surrounded by a film of liquid particles, these, forming a visible envelope, produce what is called *vesicular vapor*, and become the matter of a cloud. Our business here is with the state of vapor in the former case.

None of the writers on the subject of elastic fluids recognize the existence of *vapor in water* in its separate and independent character. It seems strange, however, that the mere appearance of the great quantity which rises out of a body of so-called hot or boiling water, *when poured out*, should not have suggested the idea that it must, previously to its escape, have existed in the water. The more so, when it is considered that without such separate and independent existence its volume could not have

been enlarged, diffusion or divergence would have been arrested, pressure nullified, and elasticity itself have ceased to exist.

In such a state of things how could these properties have been re-established? Are we to suppose that they had been *temporarily suspended* in each atom of vapor from the moment of its generation until its escape; or that, instead of being so generated at *the bottom* of the vessel, and near the source of heat, it was in fact, only formed *at the surface* and in contact with the *colder atmosphere?* Such results would be anomalies alike irreconcilable with fact and the evidence of our senses.

But, it may be asked, why the vapor, by reason of its greater levity (if it exist in the water), does not at once rise to the surface and escape into the air, which is so much more rarefied a medium? With equal reason might it be asked why the vapor, which exists in *the atmosphere near the earth's surface*, does not rise at once to the upper and *more rarefied regions*, and leave the lower without any? The cause and the reason are the same in both cases, and are to be found in the nature of vapor *as an elastic fluid* filling the entire space.

The air is but a medium, so is the water (as regards density and pressure), in reference either to the upper or lower regions of the atmosphere, or the still lower medium of the water. The whole is then but a question of degree, the vapor atoms being always in a state of mutual repulsion, irrespective of the medium in which they may be placed. It is this diffusive action which prevents any permanent irregularity, as to quantity, in any one portion, whether the medium be the atmosphere, or a fluid of any kind. This, practically, is the most important feature of Dalton's great discovery of *diffusion*, whether in reference to meteorology

or physics; to temperature in the atmosphere or in the water, to the properties peculiar to the liquid or the vaporous states.

"Homogeneous elastic fluids," says Dalton, "are constituted of particles that repel one another with a force decreasing as the distances of the centres of the particles." This law of repulsion and relative distances being general, care must be equally applicable to vapor or *steam*, as to any other *elastic fluids*. His precise statements put this beyond all doubt. They are to the following effect:

1st. That *vaporized bodies* cannot, on any scientific principle, be classed in a distinct category from *permanently elastic* fluids.

2nd. That when two or more gases or vapors are put together, either into a limited or unlimited space, they will finally be arranged each as if it occupied the whole space, and the others were not present.

3rd. That they retain their elasticity or repulsive power *amongst their own particles*, just the same *in the water as out of it*, the intervening spaces having no other influence, in this respect, *than a mere vacuum.*

If then, in reference to varying degrees of indicated temperature, the quantity of vapor *generated in*, or *injected into* a body of water, be great or small, the repulsive power among its particles will cause them so to diffuse themselves, that no part of the liquid mass shall be without its due proportion.

Professor Silliman,* under the head "Diffusion of Gases," observes:—"*Liquids* mixed together, gradually separate, and lie *superimposed in the order of their densities*, the surfaces of their separation being horizontal. When, however, *gases*

* "First Principles of Physics and Natural Philosophy," by Professor Silliman, of Yale College, New Haven.

[and so of the vapors] are mixed, they present other conditions of equilibrium, as follows:—A *homogeneous or persistent mixture* is formed rapidly, so that all parts of the same volume are composed of the same proportions of the mixed gases. Berthollet demonstrates this law of Dalton. This interpenetration or movement of gases, he called *diffusion*. This he reduces to the following law, namely:—"In a mixture of gases, the pressure, or elastic force, exercised by each of the gases is the same as if it was alone."

The question then is, whether this law does not hold, and is not equally applicable to the elastic fluid vapor.

But he brings the analogy closer by considering them as *mixed with water*. "When a gas comes into contact with *a liquid*, the gas is absorbed in a quantity varying with the pressure to which it is subjected. Thus the constituents of the atmosphere are always found in the water with which it is in contact. This he illustrates by experiments which, as he observes, demonstrate that the mixture of gases with liquids is in accordance with the following law, viz: "For the same gas, the same liquid, and the same temperature, the weight of gas absorbed is proportional to the pressure—that is, that at all pressures, the volume dissolved [mixed with it] is the same."

Now, pressure or the effect of diffusion being the same, what is the amount of that pressure *in water*, to which the vapor is subjected? This can only be determined by reference to the respective densities of the two media, the water and the air. (See ante, Vaporization.) Here then is to be found the true amount of effective pressure *from without*, to which every gas or vapor *forced* into, or *formed in* water must be subject.

But we have a still clearer view when the Professor speaks of *molecular repulsion*, namely:—"If a definite vol-

ume of air is admitted into a vacuum of twice that capacity, it does not, like a solid or liquid body, retain its *original volume*, but expands, *and fills the whole empty space.* Since an internal force is necessary to *hold together* the particles of gaseous bodies, there must be a force which acts *repulsively among their particles;* and the same offers a resistance when those particles are brought together by mechanical pressure. A similar resistance to compression is displayed in *liquid* and solid bodies."

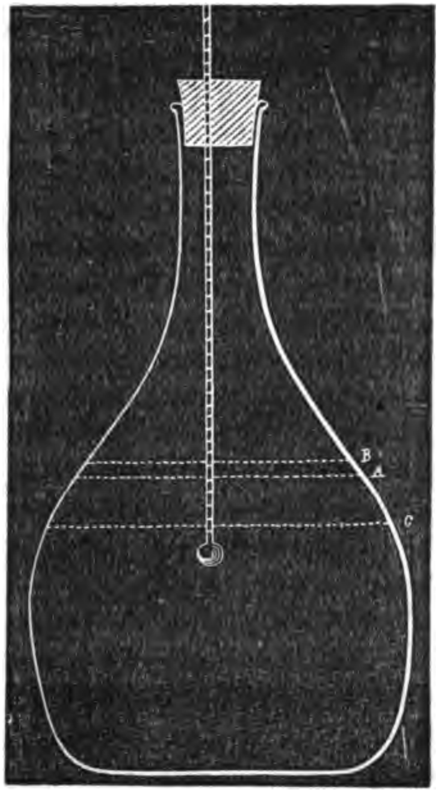
Here we have the case of molecular repulsion shown to be equally referable to that of vapor, or other elastic fluid, namely, in the force which acts repulsively among their particles.

On the elastic force of heat, he adds: "Since the *accumulation* of heat causes the atoms of bodies to *separate*, and its *removal* causes them to *approach each other*, it must be admitted that whatever may be the nature of heat, it acts *as a repulsive force.*"

It has already been observed that this repulsive force cannot be explained on any other known principle than that of electricity. Bodies (and atoms of vapor are bodies) in the same electrical state, have a mutual repellent action, as witnessed in the divergence of the leaves of the electroscope. It is remarkable, then, that this repulsive force, which is the very element of elasticity, is altogether ignored by writers when alluding to *vapor in water*, although all admit it to be an elastic fluid, and endowed with the properties peculiar to such. In illustration of the existence of vapor in water, the following experiments may be relied on as conclusive.

Four pounds of water, at 60°, were put into a tightly-corked bottle, a thermometer being inserted in the cork, as at Fig. 30. The bottle was then plunged in a tin bath of boiling water with a gas burner under it. The tempera-

Fig. 30.



ture being brought to 200°, the level was raised from A to B. On being allowed to cool, without removing the cork, the level of the water returned to A as it had been at the commencement. In this case all the heat that had gone to the formation of the vapor was subsequently lost by *radiation* from the glass, no loss of weight having taken place.*

* It is here necessary to caution experimenters not to confound two processes which are essentially different in the so-called act of *cooling*, namely, that which is occasioned by the mere loss or escape of *heat*, and the escape of *vapor*. In the first (as above), the heat was lost solely by *radiation*; in the latter, it would be lost by the actual escape of the vapor, which would carry away both *heat* and *weight*. These points are not sufficiently attended to in practice.

The next experiment will show a different result. In the same bottle the 4 lbs. of water were, as before, raised to the temperature of 200°. The cork was then removed, and the water poured into the flat pan* as already mentioned. After a lapse of sixty minutes the temperature of the water in the pan had returned to its initial 60°. On being poured back into the bottle the level was reduced to C, the loss of weight being seven ounces, and in volume about twelve cubic inches.

Here we have absolute proof of twelve cubic inches of water having been converted into vapor when at 200°, and of its subsequent escape into the air and as soon as it was allowed an adequately exposed surface.

As all authorities admit the existence of vapor in *the air*, yet deny, or ignore, it when in the denser medium of *the water*, the prevailing theory would imply that it can exist in *no other proportion* than in the enlarged, expanded state, due to the mere pressure of the atmosphere.

Doctor Reid says, "Boiling water produces steam barely sufficient to overcome the pressure of the air, and rise against it." On the contrary, steam may be seen, and condensed, on its rising out of the water, not only at the boiling point, but at all temperatures, up to, as well as beyond, 212°.

In fact, every atom of the liquid, on its being converted into one of steam, must have had sufficient buoyancy to overcome, not only the pressure of the *air*, but that of the denser medium of the *water*, or it could neither have risen in, nor out of it.

The next experiment brings this question still nearer to

* The pan was an iron one, two inches deep, and with a surface of two square feet, and being rubbed with black lead, the water, when cooled, ran off, without leaving any damp on the pan or losing any of the water.

absolute proof. Four pounds of water, as before, having been raised to 200° in the same corked bottle, and the same physical enlargement of the mass from A to B having taken place (and which will be found to be not far short of ^{1}th to $\frac{1}{28}$th, according to Dr. Ure's statement) the bottle was then removed from the water, and being wrapped round in thick warmed flannel, to obstruct radiation, was *allowed to remain in that state for one hour.* The water was then poured out into the flat pan, and the vapor allowed to escape as before. On being poured back into the bottle, the deficiency was found to be five ounces, or about nine cubic inches, representing the weight and volume of the vapor that had escaped. It would thus appear that the quantity of vapor was reduced, *by radiation* during the hour that the water remained in the bottle, by about two ounces.

The question then is, *where, and in what state, were these five ounces of vapor during the hour* when the whole was allowed to remain undisturbed? That they must have been in the condition of vapor, and in the water, in an independent state, seems indisputable, unless on the absurd hypothesis that this vapor was only formed on coming into contact with the cold atmosphere to which it was exposed when poured out; or that such *cooling process*, by some miraculous interposition, imparted not merely the enlarged volume, but the properties of mutual repulsion, pressure, force, and high temperature.*

The next experiment will further illustrate the independent existence of the vapor in the water. Two pounds

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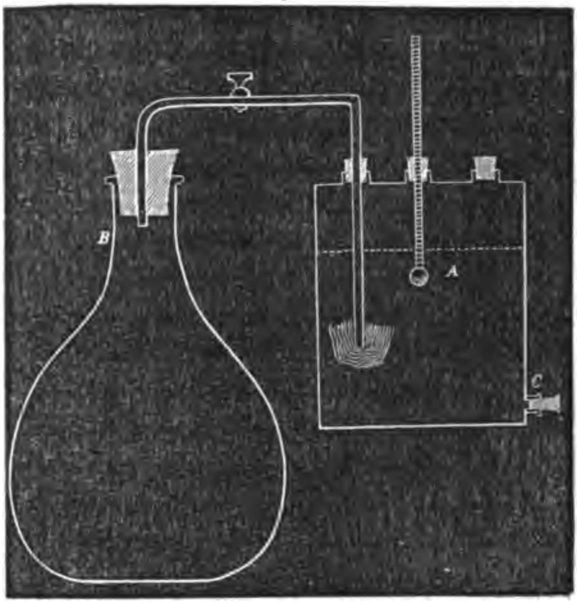
* In these experiments, where accuracy is desirable, it is better to confine the temperature to 200°, or at least not beyond 210°, as, where that is exceeded, and ebullition takes place there is considerable difficulty in knowing the exact moment when the heat should be withdrawn.

of water at 60° were put into a glass vessel A with three tubulures, as in Fig. 31. Steam was raised in a separate

Fig. 31.



vessel B (under which a gas-burner was placed), and passed into it until the thermometer indicated 200°. The water was then let out by the tubulure C, by withdrawing the cork, and run into the flat pan. The vapor then rose in large volumes, and after about one hour, when it had returned to its initial temperature, the water was weighed, and the increase of weight was found to be five ounces.

There is considerable difficulty in knowing the precise weight of vapor remaining in the water in these experiments, as there is a continued loss of heat by *radiation* during the entire process, consequently, the weight of water and vapor *remaining* must be greater in proportion to such loss of heat. The best mode is to have the water

well filtered and distilled, and then raised to 200° or 212° *without ebullition* or loss of its vapor. When this is done, the amount of evaporation from the water in the pan will be about 1½ to 2 ounces per pound of water, according to the accuracy with which the experiment is performed.

Thomson says: "Water is converted into vapor at all temperatures, even at 32° or lower. But the elasticity at low temperatures is low, and it increases, as the temperature increases, till, at 212°, it is equal to that of the atmosphere."* It would have been more correct to say that, as the elasticity must be in proportion to the quantity present, so must be the temperature. Indeed, he himself says: "Thus the quantity, and, consequently, the elasticity, is continually increasing with the temperature of the water;" rather with the quantity existing in that water, and by which the temperature is effected.

Again he adds: "We see that elasticity increases at a very rapid rate with the temperature. Attempts have been made to represent this increase by empirical formulæ, so as to enable us to calculate the elasticity of the steam for any given temperature." To this he adds, with great truth: "But such formulæ, from the imperfection of the data on which they are founded, cannot be accurate;" a remark which might have a more general application.

Quantity (of vapor) then virtually becomes the cause and the measure both of volume and pressure. They are, in fact, correlative terms. This view of the subject, which is confirmed by all physical tests, takes it out of the hands of the mere mathematician, who has hitherto so overloaded the inquiry as to render it difficult for practical engineers to arrive at practical truth or certainty in the investiga-

* "Of Heat and Electricity," by Thomas Thomson, M.D., Professor of Chemistry in the University of Glasgow.

tion of nature's laws, as regards the relation which heat bears to water or steam.

But Thomson, though justly considered a high authority, appears to have fallen into the common mistake of connecting the *generation* of vapor with the *boiling point.* He observes: "Papin, an ingenious French physician, who resided in London, contrived a vessel to which he gave the name of *digester.* If this vessel be half-filled with water, and the lid screwed down tight, and if it be then set upon burning coals, a portion of the water is soon converted into steam. *This conversion begins at the boiling point.*" Here is a manifest oversight as regards the generation, or conversion of liquid atoms into steam. And, we see further, how the one error leads to another, when he adds: "But the elastic vapor [which had so began to be formed at the boiling point] being confined, presses upon the surface of the water, and thus prevents the conversion of any more of it into steam, till the *temperature of the water* rises above the boiling point. This heat being conveyed to the steam, it now becomes capable of bearing another portion of vapor without being condensed into water."

There are here many errors. Instead of the elastic steam *pressing downwards upon the surface of the water*, with equal correctness it may be said that the vapor, in the water, equally presses *upwards*, *against the steam above it.* Thomson altogether overlooks the existence of vapor in the water, and is led into the singular idea that the heat is first received by the water, which is thus made hotter, and then renders the steam above the water capable of bearing another portion of vapor. He does not, however, say where that additional portion of vapor is to come from.

As the apparent difficulty of conceiving the existence of vapor in water, in an independent elastic state, continues

to prevail, it will be necessary to examine the subject in another point of view.

Dalton observes, "Vapor exists at all times in the atmosphere, and is one and the same as steam or vapor at 212° or upwards." This is an important fact, and goes far to confirm the case of *unity* of heat, as already explained, seeing how infinitely minute must be the equivalent of heat in each atom of vapor while in the atmosphere; and that 212° is merely the result of a given quantity or number of such accumulated atoms then present in a given space.

Practically, then, there would appear a greater difficulty in conceiving the existence of vapor in the *atmosphere*, than in the *water*, seeing that air is a positive refrigerator and conductor of heat, whereas we have no reliable grounds for saying that a liquid is the former, and we know it is not the latter.

Desaguliers (who appears to have been Watt's first instructor), while controverting the hypothesis of his predecessors, who taught that there was a *chemical* union between the air and the vapor, proposes his own, namely, that, "water is capable of being converted into an elastic fluid much lighter than air." On this Dr. Henry, the biographer of Watt, observes: "This notion which gives identity to vapor formed by heat in a vacuum is ingenious; but how the vapor should *ascend till it arrives at the air* of the same density is not very conceivable." Certainly not, so long as the heat is assumed to be absorbed *by the water* while still retaining its liquid form, and that vapor cannot co-exist with water in a separate and independent capacity. So soon, however, as these errors are repudiated, the ascent of the "vapor" through the liquid mass will be as intelligible as the ascent of a cork from the bottom to the surface in a vessel of water.

Here, then, the question is raised—here is the stumbling-block which has so long stood in the way; yet, if we only look at the formation of vapor, not in the mass, but with reference to its atoms, separately and successively receiving their quotas or equivalent units of heat, and obtaining their distinct properties of levity, repulsion, and elasticity, all difficulties will be at an end.

Let us here keep in mind Dalton's clear and now recognized theory (see page 57), and apply its leading principle, by which we are warranted in recognizing the independent status of vapor as an elastic fluid. This may be illustrated in the following way: Let A A represent a given number of grains of shot, sufficient to cover the bottom of a glass beaker, as in Fig. 32. In this case gravity will be the sole agent as regards position.

Let us next suppose these grains to become endowed

Fig. 32.

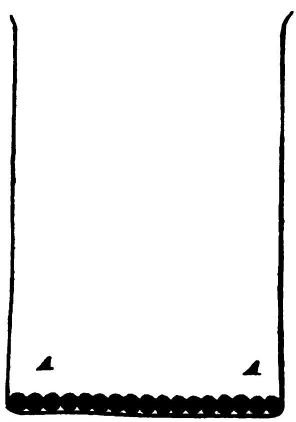
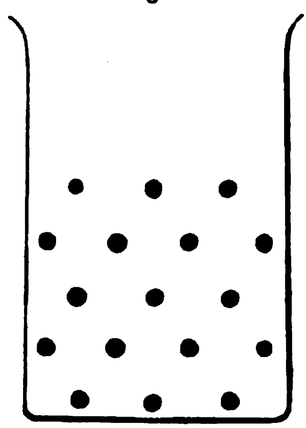


Fig. 33.



with the property of mutual repulsion, under Dalton's law, as above, and as the pith balls under electric influence. By virtue of these properties they would not only ascend,

but *separate and diffuse* themselves through the entire space or area of the vessel, and take their places respectively, as shown in Fig. 33.

Here the case is strictly analogous to the bottom stratum of atoms of water, as shown under the head of *Vaporization*, when converted into atoms of vapor. Under such circumstances they would fully satisfy Dalton's law retaining their repulsive powers—filling the entire space—and producing homogeneity of temperature. We, then, rightly infer that vapor follows the law of diffusion, whether it be in the medium of the air or that of the water.

A recent cyclopædic authority observes: "When a quantity of water is heated several degrees above the boiling point *in a digester*, if a hole be opened the steam rushes out with prodigious violence, and *the heat of the water* is reduced in the course of three or four seconds to the boiling temperature. The quantity of steam that has issued amounts to only a few drops [a strong proof of the myriads of atoms of vapor of which these few drops were composed], yet they have carried off with them *the whole excess of heat from the water* in the digester."

We have here a striking illustration of the confusion arising from the hypothesis of considering the *water* as *the recipient of the heat*, instead of looking to the *vapor*. This vapor existed below the surface level of the water as well as above it in the digester, and as both went off together none would be left behind in the water, but the saturating quantity, which can only be removed by evaporation in the ordinary way. The so-called "excess of heat" carried away, was then solely that which had constituted the steam, and which the increased pressure in the digester had *mechanically* compelled to remain and accumulate *both above and below* the surface level of the water.

But see how the error of this theory may be detected. If the few drops of water had carried away the whole excess of heat, where, or in what, had that heat been deposited? Was it in the *water* or the *steam?* If the steam, which had "rushed out with such violence," had carried away that excess of heat, it leaves us in the dark as to the means by which the heat, *said to be in the water*, was so reduced as to lower the temperature to the boiling point. We have, in fact, no alternative but admiting that steam must have been present both *in the water* and *above it.*

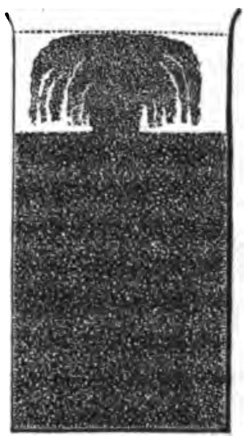
The same authority, feeling the difficulty of the position, adds: "The solution [diffusion] of a given quantity of water (in the form of vapor) in air, has an effect on its specific gravity. So the laws, according to which the humidity of the air varies in proportion to its condensation and rarefaction, form an entirely new subject, in which our ideas have hitherto been vague or erroneous." This vagueness will be found to arise from considering vapor, air, and water, in reference to their *masses*, rather than their elementary divisions.

In proof not only of the presence of vapor in water, in the aggregated state of globules, but of their great ascensional power, the following experiment is conclusive. It is true, as Dr. Robinson observed, "an experiment does not establish a general proposition, and never can do more than prove a particular fact." The *proving a fact*, however, is a decided step in actual progress, and enables us to draw inferences and illustrations which, leading to other facts, bring us ultimately to absolute demonstration.

Into a glass beaker put as much pounded coal (Fig. 34) (see experiment No. 29, pages 122, 123) as will fill it to a depth of three or four inches. Pour into it as much boiling water as will nearly fill it. Let the flame of a spirit-

lamp be then placed beneath, and touching the centre of the bottom. The result will be that the globules of vapor, on being formed, will rise with such rapidity as to force their way through the mass of coal and project a portion of it high in the water. This experiment, so unlike any that have been referred to by previous writers, shows how far we still are from having a correct view of the union of heat with the element of water, and though it may only prove a single fact, leads irresistibly to the following conclusions, namely: 1st, the absolute existence of vapor in the water; 2d, the formation of globules or aggregates of that vapor; and 3d, the great ascensional power of those globules.

Fig. 34.



As the view taken in this treatise of the existence of vapor in the water is so opposed to the theory of all writers, we cannot be too strict in examining the statements made by them. Among these may be selected the Comte de Pambour, as one of recognized authority; and the more so as he has gone much into detail on the subject, and has supplied a sufficient number of texts under which the theories of the present day may be examined.

THEORY OF PAMBOUR.—Speaking of "the constituent heat of steam *in contact* with liquid," he observes: "There is an inquiry relative to the properties of steam which has long fixed the attention of philosophers: it is that of the quantity of heat necessary to constitute the steam in a state of elastic fluid under various degrees of elasticity." We here find steam regarded as in the mass. Had he spoken of the equivalent quantity of heat necessary to constitute

each single atom of steam, as one of the constituent integers forming an elastic fluid, the inquiry would have been legitimate and practical. Had the subject been pursued analytically, he would have found that "*various degrees of elasticity*" was only another term for *various quantities of vapor.* With equal propriety he might have asked what quantity of water a gallon vessel would hold at various degrees of weight.

The very use of the term *an elastic fluid*, as if he were speaking of a sponge or other expandible material, is evidence of his regarding steam *in bulk*, and as a single body, and apart from its elementary atoms.

Repeating the old theory, he adds: "It is well known that when water is evaporated [vaporized] under atmospheric pressure, in vain new quantities of heat may be added by means of the furnace; neither the temperature of *the water* nor that of the *steam* can rise above 100° of the centigrade thermometer, or 212° of Fahrenheit. All the heat which is incessantly added to the liquid must pass *into the steam*, but must subsist *there* in a certain state, called *latent;* because the heat, though really transmitted by the fire, remains, nevertheless, without any effect upon the thermometer." We have here a series of errors, illustrative of the prevailing theories: let us examine each:

"In vain new quantities of heat may be added." So far from being *in vain*, every additional unit of heat was palpable in the formation of additional atoms of vapor; and as available in dynamic effect as the first, or any subsequent unit taken up. Had these additional quantities of heat *been retained* in the vessel, they would necessarily have had a commensurate effect on the thermometer. Instead, then, of looking in the right direction—namely, to the formation of the new steam which was seen continu-

ously rising and escaping into the air, and carrying away its respective quantities of heat—he looks to the water, which, so long as its atoms remained liquid, could not have possessed it.

If the thermometer could have spoken, it would have asked, how it could have received any additional temperature from heat which had passed away in the new vapor; and that if the object was to act on the thermometer, that vapor should not have been allowed to escape. As well might Pambour himself affect surprise on experiencing no effect from the fire of a whole platoon whose balls had passed him by, or that the target should have escaped perforation from arrows that passed above it. The simple fact is, that the newly-formed vapor which receives and carries away the newly-applied heat, *does not reach or come into contact with the thermometer* bulb, and therefore, can have no influence on it.

With equal propriety might it be said that, "*in vain*" fresh quantities of water were continuously poured into a vessel *already full*, and then wonder that there was no increase in weight—the truth being that the water, at the so-called boiling point, is then absolutely full of vapor, in the true Daltonian sense of the term. (See Fig. 9, page 61.) Vapor will certainly be made, if more heat be applied, but no more can find room in the water in the vessel, unless by *mechanically confining it*, as was done in the case of the digester; or, as when we inject more water into the hydraulic cylinder without enlarging its area.

The oversight, then, consists in looking in the wrong direction for the results of "the new quantities of heat added from the furnace," namely, to the *water*, in its liquid form, in the very face of the vapor carrying that heat away.

Pambour says: "All the heat which is necessarily added to the liquid must *pass into the steam.*" Certainly not into the steam *then existing in the vessel*, but into the successively formed atoms, as rapidly escaping as they are generated.

Again, "But it must continue *there* [in the steam] in a certain state, which is called latent." This is altogether erroneous: 1st, as it would imply, not that new or additional steam was generated, but that the heat merely went to give greater volume to *that already formed;* and 2d, that this newly-applied heat would be *latent*, whereas its sensible or dynamic value is at once perceptible in giving the newly-formed vapor the properties of elasticity, pressure, and thermometric effect.

He then goes on to observe, "This latent heat evidently serves to maintain the *molecules of water* in the degree of separation suitable to their *new state of an elastic fluid.*" Here we have the anomaly of considering the molecules of water in a double capacity of a *liquid* and an *elastic fluid.* Besides, there can be no such *separation* of the *molecules of water:* no such thing as the suspension of that *mutual attraction* which is the inseparable condition of liquidity. When they cease to be mutually attractive, they cease to be liquid.

"But," he adds, "it is important to know the quantity of latent heat, in order to appreciate, with accuracy, the modifications which steam may undergo." There is great confusion here. Steam can undergo no modifications by *latent* heat, whatever it might by *sensible* heat, after it has received its property of elasticity, and is absolutely steam.

The following will show the numerous imaginary dis-

tinctions which ingenuity may suggest to complicate the subject and embarrass the inquiry after truth:

With the view of ascertaining the specific laws which regulate the action of steam, the Comte examines the subject under the following heads:

"1st. The relation between the temperature and pressure of steam *in contact with liquid.*

"2d. The relation between the relative volumes and the pressures, or between the relative volumes and the temperatures, at equal pressure, in the steam *separate from the liquid.*

"3d. The relation between the relative volumes, the pressures, and the temperatures in the steam, *in contact, or not in contact*, with the liquid.

"4th. The direct relation between the relative volumes and the pressures in the steam in contact with the liquid.

"5th. The constitutive heat of the steam in contact with the liquid.

"6th. The conservation of the maximum density of the steam, during its action on the engines."

In the mathematical investigations which follow these heads, we have sufficient to satisfy the most enthusiastic advocate of equations and formulæ, and the author revels in pursuit of the game he has thus raised. In such an *embarras des richesses* he may well be excused, if he sometimes gets seduced from the truth, or loses his way in the algebraic cloud he has created. He is prodigal in the several distinctions here drawn between pressures, volumes, and temperatures of steam, *in contact*, and *not in contact*, with the liquid, and, as it were, for the mere play of ingenuity and power in reducing results to what he calls *practical formulæ* in full algebraic costume, the reading of which he considers *easier than if given in words.* That

it is not so easy, however, is manifest from his own conclusion, when he says that he found it necessary to annex *rules designed for persons not familiar with algebraic signs*, and intended to render the use of the formulæ contained in the work perfectly clear and easy. Nevertheless, it may well be credited that no practical engineer has ever examined the distinctions he has so elaborated between his own formulæ and those of Dulong, Arago, Tredgold, and Southern. It may even be doubted whether they have ever been consulted by any one of those professional manufacturers for whose special benefit he tells us they were written.

In truth, he anticipates this very result when he adds, "Among the persons who are engaged in the construction or in the working of steam-engines, and whom this work may consequently interest, there is a great number to whom algebraic terms are little familiar, and who usually [we may say invariably, if they have first to study his rules] give up the reading of a book as soon as they perceive it steps beyond the simple notions of arithmetic." It need only be said that, after an extensive inquiry, we find this to be verified to the letter.

Again, he says:—"When it is intended to make a work profitable to those persons, the usual practice is to annex to each of the definitive formulæ *an explanation in full words* of the arithmetical operation which it represents." No more forcible illustration could be given of the *practical inutility* of his own formulæ. Feeling the truth of this, he adds:—"With the number of formulæ contained in this work such a proceeding would become *impracticable*, since the explanation of *each series* of formulæ would require a considerable number of pages." With such a

confession, how could he have imagined that a single one of his series had ever been read?

In his appendix he has given "Rules for reading his formulæ," and to which he naively adds:—"*When once a person shall have made himself master of them* he will be capable of reading not only the formulæ of this work, but of all that may present themselves in other works." The very task of studying this, his rudimentary treatise on algebra, in order to be competent even to read his formulæ, is as if he had sent them back to school to learn to be competent algebraists. It may, then, be said, that not one in a thousand of those for whom the work is professed to have been written are, as he observes, "familiar with algebraic terms."

Pambour thus introduces this table of the relative volumes of steam, generated under different pressures, in comparison with the gross volume of water from which it was produced, namely: "When we speak of steam generated under a given pressure, we understand the steam considered *at the moment of its generation*, and, consequently *still in contact with the liquid.*" It is here manifest that he considers steam in the mass, instead of in its elementary molecules. Now, there is no such moment as the generation of a body of steam; although there is such a moment peculiar to the formation of each individual atom. The very idea of steam being "consequently in contact with the liquid" is proof of this, as the attaching any distinct thermometric temperature, volume, or pressure, could only have reference to the steam in the mass. Yet, as already pointed out, pressure, temperature, and volume, are the mere resultant co-efficients of *quantity* or progressive accumulation. As well may he have referred to the height or pressure on its base of the Cathedral of St.

Paul's, at the ***moment of its being raised*** in the form of a building, and as if its elevation or pressure were a sudden or single operation, the weight or pressure being (as of the aggregates of steam) the ever-varying and increasing result of the accumulation of individual blocks. Tables of pressures or temperatures, made or based on such non-existing conditions, can necessarily lead to no practical results, and may be regarded as mere scholastic devices. The idea of steam undergoing changes in volume and density, while its temperature remained the same, as he observes, could never have been entertained, had he looked physically or analytically into the process of nature which ensues when heat is applied to a liquid.

Adopting the expansion theory, Pambour is driven to the expedient of supposing that steam is generated in the mass, from water in the mass, after it had been raised to a given temperature, say 212°, and it is in that sense that he speaks of considering steam at "the moment of its generation, and in contact with the liquid."

On the subject of written authorities, and the variety of views and theories (a list which might be still further extended), enunciated by the highest and most justly esteemed writers, and looking to the proofs which may be offered as to their common mistakes, we may justly infer the risk of relying on any until a closer and more scientific inquiry has been made, and until, in parliamentary language, the whole subject has been referred to a committee up-stairs. The present treatise will at least have this merit, of enumerating sufficient grounds for such an inquiry, and of leading to a view of the subject more in accordance with nature's laws and physical demonstrations. We may, then, without the imputation of dogmatizing, ques-

tion a reliance on any thing that has hitherto been written on the subject here under consideration.

So far from looking to what are called "high authorities" as safe, practical, experimental, or scientific guides, we have in their writings on this question but a melancholy array of contradictions and anomalies, from which we practical men can find nothing on which we can rely, and are compelled to admit the necessity of experimenting and reasoning for ourselves, if we would avoid these discrepancies which embarrass both the subject and the student.

SECTION VIII.

OF CONDENSATION.

"The term *condensation* is commonly applied to the conversion of vapor into water in the process of distillation. The way in which vapor commonly condenses is, by the application of some *cold substance.* On *touching it*, the vapor parts with its heat, and doing so, it immediately loses the proper characteristics of vapor, and becomes water."—(*Encyc. Brit.*)

We have here a correct description both of the cause and process of condensation, or the reconversion of vapor into the state of a liquid. Let us then apply it to the condenser of the steam-engine. The steam, by virtue of its elasticity, rushes into the condenser as it does into the worm of the still, but what "*cold substance*" does it there encounter? *This is the all-important point of the inquiry.* The universally received theory is, that meeting a body of *cold water*, it imparts its heat to the latter, and is thereby instantly condensed, or reconverted into the liquid state.

One of the objects of this treatise is to show that this theory is altogether fallacious, and that water is not a *substance* to which vapor can give out its heat. It will, doubtless, hereafter be a matter of special wonder how long we have confidingly adopted a theory without inquiry, in the face of the many anomalies which it presents, arising from the assumption of water being on the one hand an *absorber* of heat, and on the other a *non-conductor* of heat.

In common with others, the writer of this treatise formerly considered that heat was absolutely absorbed by the

water, which still retained its liquid form, and that the vacuum produced in the cylinder was rightly attributed to the steam giving out its heat to the water. It was only under an irresistible conviction, arising out of numerous experimental proofs, that he abandoned the error under which we have been so long laboring.

We will first examine the statements of recognized authorities, and the grounds on which they are based. Condensation in the steam-engine is thus described by a fully competent engineering authority:—"The condenser is the most wonderful part of the steam-engine It is here that the whole process carried on in the boiler, in so great bulk, and at so much expense, is *instantly reversed*, and all its laborious effects at once, as it were, *annihilated.* It is the *instantaneousness* of condensation that is its value.*

To this description is added an elaborate statement to show that the vacuum formed by the assumed *annihilation* of the steam may even be "*too good.*" In speaking of the efforts of rival engineers when extolling their respective engines, the same author observes:—"It is to be regretted that time and talent should be thus wasted." No doubt —but the formula the author has given is no proof of the soundness of the principle or assumed truth it affects to establish—the more so if such formula should prove to be but a fiction. "Such," continues the same author, "is Mr. Watt's simple account of his beautiful invention, the *condenser or refrigerator*, which is the characteristic member of the modern steam-engine."

The condenser and refrigerator, we see, are here taken as synonymous. Nevertheless, though a *refrigerator*, as in distillation, must be a *condenser*, the condenser of the

* "The Steam-Engine." By John Scott Russell, M.A., F.R.S.E.

steam-engine, as regards the action of the cold water, is not a refrigerator, the water injected having no *direct* influence in depriving the steam of its heat, or converting it into water. That such, however, is the theory adopted since Watt's time is thus shown by the same authority.

"The third member, added by Watt, is a refrigerator, or condensing apparatus, perfectly separate from, and independent of the other two, for *reconverting the steam*, after it has done its duty in filling the cylinder into the liquid form from which it had originally been formed. We have, then, the boiler, the cylinder and the *refrigerator* or *condenser*."

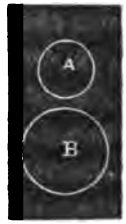
This is a correct account of Watt's apparatus, and the theory it involves. In disproof, it is here to be shown that the constituent heat of the steam is not diminished by contact with the injected water—and that the water does not take up or absorb a single unit of the heat possessed by the steam.

Watt's views may be thus summarized in his own words, viz.: 1st. That the elasticity of the steam in the cylinder would cause it to rush into the separate vessel—the condenser—to restore its equilibrium. 2nd. That in meeting the jet of cold water, the steam, giving out its heat, would be reduced in temperature and volume. 3rd. That this reduction in volume would be from 1,728 cubic inches (the assumed proportion under atmospheric pressure) to that of its previous bulk as water.

In the adoption of this process, a sufficient, and to him, satisfactory result, having been obtained, any further inquiry would have seemed unnecessary—the inference naturally being, that the cold water had the direct effect of reducing the steam to its previous state and volume, as a liquid. This, however, natural as it may be, will hereafter

appear to have been a mere unwarranted inference. Let us examine this closer.

Let A represent an atom of water, and B an atom of vapor, the enlargement in bulk being $\frac{1}{24}$th. It has already been shown (page 83) that if A has no conducting power, neither can it have a receiving one. Our great lexicographer says, "giving is a relative action, and requiresta correlative to answer it; giving on one part transfers no property, unless there be an accepting on the other." So of giving out heat by B to A; none can be conveyed unless there be a correlative power of receiving it in A. This, taken in conjunction with the argument at page 83, presents a *prima facie* impossibility as regards the vapor atom giving out its heat to the liquid atom.



But, for a moment, let it be assumed that the latter, A, had the power of receiving and retaining the heat from the former, B; it would then be but a mere reciprocal change. A would have become the vapor atom, and B have returned to the state of a water atom. If, then, B (the vapor) met no "cold substance," to which it could give out its heat, *it must retain it;* and this is the case with steam in water.

Looking practically at the state of the injected water in the condenser, the mere fact of its indicated temperature being 100° as Watt described it, shows that the steam was not condensed into water, but was merely diffused through the mass, seeing that each pound weight of water at 100° contains one ounce of vapor, and which it has been shown may be recovered if allowed to escape.

If, indeed, water could convert vapor into the liquid state, by abstracting its heat, the inevitable result must be, that vapor could *never be formed*, or at least, have any

dynamic effect. The moment the first atom of the liquid was *converted* into one of vapor by the heat, it would as instantaneously be *reconverted* by *the mass of water surrounding it.* It appears impossible to reject this inference Thus, no continuance could exist, and no body of vapor could be formed.

That vapor has not been reconverted into water, or *annihilated*, is at once established by its rising out of the water with all its properties and characteristics unimpaired, and, so to speak, appearing in *propriâ personâ* in disproof of such annihilation. It has already been shown that when water is placed in a glass beaker, with a gas burner under it, and a glass saucer or large dial (also containing water) is placed on the former, the rising vapor will exercise its power, and increase the so-called temperature of the water in the saucer from the first minute after the heat has been applied to the beaker. *In limine*, then, we are compelled to admit that the heat has not been retained *in the water*, but has passed away in vapor.

What really takes place is this,—when vapor is thrown into what may correctly be called an *atmosphere of water*, each atom is at once compressed, or reduced in influence, and prevented exercising its full expansive power by the combined densities of the two media—the water and air; no diminution, however, of the temperature of the vapor atoms follows. They merely remain, with their compressed volumes in the water, until they escape into the atmosphere, or by contact with some *cold substance* lose their heat, and are then *bona fide* reconverted into the liquid form.

Water then, or, indeed, *any liquid*, cannot be considered as a substance to which heat can be imparted. In a word, heat cannot be received *and retained* by *liquid particles*,

each of which is susceptible of an instantaneous change, in its own statical or electrical condition, by the accession of heat.

As well might we expect that atoms of ice could receive and absorb heat, and have their temperature raised, yet *still retain their crystallized form and status of ice*, as that those of water could receive it, and retain their *status of liquidity*.

Air is an elastic fluid, and a recipient of heat, since its *status cannot be altered* by it, there being no *fourth state* into which it might enter by a further accession of heat. Besides, being also a *conductor* of heat, it is capable of receiving and imparting it to others, from atom to atom. In this way the vapor in the atmosphere, when brought into contact with a body of colder air, and more or less of the vapor atoms (according to the amount of atomic contact realized between them), gives out its heat to those of the air, returns to the liquid form, and produces the effect of visible clouds.

When, also, we consider the extreme miscibility of elastic fluids, or æriform matter, and the extent of surface for mutual contact presented by the aggregation of the myriads of atoms which compose bodies of air and vapor, we can readily account for the rapid condensation of the vapor atoms in the atmosphere, when brought into connection or collision with currents of colder air. To these currents, then, may be attributed all atmospheric changes of temperature and humidity, from clouds, fogs, and rain, up to the more rapid discharges accompanying electric disturbance.

If, also, we look to the change in the electric condition of each of these vapor atoms, on losing the property of repulsion concurrently with the loss of heat, and thus

becoming negatively electrified, we have a key to the great quantity and intensity of the electric fluid that will be set at liberty.

The *Encyclopædia* continues—"If heat be withdrawn from steam or vapor, it no longer remains in the vaporous state, but resumes a liquid form. In this state it undergoes a great diminution of bulk;—a large volume of steam forming only a few drops of liquid. Hence the process by which the vapor passes from the æriform to the liquid state is called condensation." Here the return to the state of liquid is correctly attributed to its loss of heat, while it leaves the main questions still untouched—namely, *by what means* has this heat been abstracted, and *to what* has it been transferred?

We see that on the ordinary theory it is assumed that the *cold water* has absorbed the heat, thus reducing it to the state of liquid: in a word, "*annihilating it*," *as vapor*. This error must, therefore, be abandoned before we are in a position to proceed; for *until the true recipient of the heat be determined*, we must remain in the dark as to the principle on which condensation is affected, and the best means of producing it in the steam-engine.

In describing the difference between the condensing and non-condensing engine, the assumed rapid conversion of the steam into water is thus unequivocally asserted. "In the high-pressure steam-engine," observes Mr. Russel, "the steam is discharged from the cylinder by allowing the entering steam to press the piston on the out-going steam, and force it through the eduction pipe into the open air. In the *condensing engine*, *the steam is annihilated* almost instantaneously—a vacuum is formed upon one side of the piston by the annihilation."

This, again, raises the question, whether such annihila-

tion does, in fact, take place, and what is the substance to which the heat would be transferred?

In the case of the still, it has been shown that condensation depends on the capability of the *metallic refrigerator* to receive the heat, while the rapidity of the process depends on the extent of the surface area of such receiving substance—the cause of the so-called annihilation of steam, however, still remaining to be determined.

The unsatisfactory state of the question, as to the best system of condensation, is sufficiently shown by the number of patents which continue to be taken out on the subject, and the increasing interest exhibited in discussing the respective merits of what are termed *surface condensers*. Hall's well-known system was a true surface condensation —identical with that of the still, and would have been as unobjectionable in practice as in principle, but for certain mechanical difficulties which were found irremediable in its application.

The rapidity with which vapor parts with its heat is truly remarkable, though not sufficiently attended to in practice, while it furnishes a strong confirmation of the view here taken—namely, of vapor being a mere aggregate of atoms, each of which has its *unit of heat in combination*, all being capable of parting *simultaneously* with these respective units; for whatever may be the myriads of such atoms, the result would be equally instantaneous; hence the importance of the extended surface, or units of surface, for contact.

Faraday, in reference to the process of condensation, and the reconversion of vapor into liquids, thus refers to the action of the still: "The vapor having reached the worm, is there to be condensed; the worm is, therefore, put into a tub, and surrounded with cold water, the low tempera-

ture of which (the worm) causes the substance to *lose its elastic form*, and flow out in the liquid state."

Here the heat is correctly shown to be transferred to the *metallic refrigerator*, by true surface condensation. We have, still, however, to consider the effect when the vapor is brought into contact with a *liquid*, as water.

When steam is injected into a body of cold water, the heat is not taken from it, as in the case of the still. The steam is merely diffused through the water, its atoms becoming arranged at distances, in proportion to the quantity or bulk of water present. We also find its several atoms reduced in volume, in the ratio of the density of the liquid medium into which they have been passed, whether that be water, alcohol, sulphuric acid, or any other liquid.

In all cases the indicated temperature in the water shows a rapid homogeneity in the mass. If, then, with Professor Silliman, we consider this homogeneity to be the result of the mutual repellent principle among its particles, the whole becomes at once intelligible.

Watt's theory being so universally adopted, it is worth quoting here in his own words. Writing in the third person (as instruction to his counsel in reference to the opposition his patent experienced), under the head of "A Plain Story," he has thus given the history of his great discovery, and the use and effect of the *separate vessel—the condenser.*

He laid it down that—"To make a perfect engine it was necessary that the cylinder should be always as hot as the steam which entered it, and that the steam should be cooled down to below 100°, in order to exert its full power." This reference to 100°, which, nevertheless, seems inconsistent with a perfect vacuum, was probably

occasioned by finding that that was, practically, the lowest temperature he was able to obtain, without a further and inconvenient quantity of water. He then concludes:—"The gain would be double. First, no steam would be condensed on entering the cylinder, and secondly, the power exerted would be greater as the steam was more cooled. The *postulata*, however, seemed incompatible, and he continued to group in the dark, misled by many an *ignis fatuus*, till he considered that steam, being *an elastic fluid, it must follow the law of its kind.*"

Here we have the germ of his success. Here the inductive power of mind, and the reasoning faculty of the philosopher were applied in the legitimate way—to practice. Had he, however, then known what the true properties of an *elastic fluid* were, as Dalton subsequently discovered, his peculiar power of observation would, no doubt, have led him to avoid the oversight into which Dalton himself fell. He would have found that one of those properties was, that the water, into which the steam was thrown, by one of "*the laws of its kind*," so far from receiving the heat of the steam, and having its temperature raised to 100°, merely *acted the part of vacuum*, into which the steam would pass, without losing any of its heat, or causing a departure from any of the laws of its kind, particularly that of the mutual repulsion among its particles.

Finding, however, in the use of the separate vessel the means of rendering the *postulata* no longer incompatible—namely, the keeping the cylinder hot, and cooling the steam—nothing further would appear to him necessary, and the reconverting the steam into water would become, in his view, a practical certainty.

Dr. Robinson observes—"Watt struggled long to con-

dense with sufficient rapidity, without injection." When, therefore, he found that with it he was enabled to effect his purpose, nothing could be more natural than the attributing his success to the direct action of the cold water on the steam. Watt thus described the process:— "If there were two vessels (A and B) of equal dimensions, the one (A) filled with steam, and the other (B) exhausted, if a communication were opened between these vessels, the steam would rush from the full one into the empty one, and they would both remain half exhausted, or be filled with steam of half the density."

Here we have a correct and practical application of the law of diffusion among the particles of an elastic fluid. The other *postulatum*, however, remained unsatisfied—namely, the cooling of the steam, and reconverting it into water. "If then," he continues, "into the second vessel (B) an *injection of cold water* were made or cold water *applied to the outside* in sufficient quantity, the portion of steam which it contained would be condensed, or reduced to water."

It is clear Watt relied on the cooling effect of the *metallic condenser* itself for the conversion of the steam into water, and rightly made no distinction between the applying the water to the *inside* or the *outside*. Here he was unquestionably right; but, as regards the direct action of the water, in cooling the steam, he was in error. He, however, found it easier to inject the cold water into the condenser, and adopted that plan. We will hereafter examine the cause of this so-called vacuum, which produced the desired effect.

"The idea once started," he adds, "the rest immediately occurred. The vessel A was supposed to be the cylinder, B the condenser. The water, air, etc., accumulated in B,

could be discharged, and drawn out by a pump. The size of the condenser he assumed at random. All this passed in his mind in a few hours, and the work was complete. In a few days a model was made and at work.

In refutation of the theory of *water* condensation, may be mentioned the fact, that when steam is injected into cold water, in a separate vessel, so far from being condensed or reconverted into water, it appears to issue with the same visible cloud-like forms already alluded to in the case of vaporization, when the vapor is originally formed in the water. If cold water had reconverted the steam into a liquid state, how are we to reconcile or account for its reappearance? Why did not the mass of cold water at once cool down, *annihilate*, or reconvert the first and succeeding atoms as they were introduced? These, and other anomalies, are enough to shake all confidence in this condensing or annihilating theory. The fact that a small jet of steam discharged into a comparatively large body of water is capable of almost instantly raising the whole to 212°, shows that the process is not one of liquefaction of vapor, but of saturation of the fluid medium with vaporous atoms.

Dalton's statement, now so universally adopted, at once removes the difficulty—namely, that steam is but dissipated and diffused through the water, as if it were *a vacuum*, and being an elastic fluid, it retained its properties irrespective of such medium. The ordinary theory of condensation by water, and Dalton's theory, of diffusion, are then opposite and contradictory. Either the one or the other must be erroneous. If the ordinary theory be right, the steam would be at once annihilated by contact with the water. If Dalton's were right, it would merely be diffused

through the liquid medium, as if that medium were a vacuum.

Professor Rankine has correctly stated the law common to all elastic fluids, although it directly contradicts his own account of the cause of condensation, when he says, "The condenser is a vessel in which steam (an elastic fluid) discharged from the cylinder is *liquified* by a *constant shower of cold water.*" Now, if that were the case, it would not be "independent of other masses within the same spaces," as he has elsewhere stated.

Following the received theory, Dr. Reid observes, "Introduce into a vessel of water a tube connected with a boiler producing steam. The steam is *condensed suddenly*, and with a loud noise, as it enters the cold water, from the movement induced by it in the water." This, it will be seen, is the experiment already mentioned. So far, however, from the steam losing its heat, or being reconverted into water, it is merely transferred to a denser medium, in which each atom was collapsed or compressed in the ratio of that density. Further, the *loud noise* was not as there described, but arose from the sudden return of the water into the vacant space caused by the collapse, as the report of the gun on being discharged is the result of the return of the air.

The collapsed steam (collapsed, but not altered), subsequently rising and reappearing, on being liberated into the air, with all its properties unimpaired, is, as already observed, a substantial proof of its non-conversion into water. If, on the other hand, it had been so converted, its existence, as vapor, would have been at an end.

We have now to consider the means by which the vacuum is produced in the cylinder of the steam-engine.

SECTION IX.

OF THE VACUUM.

From what has been said in the last section, on condensation, it will be seen that according to the prevailing theory, steam, on being passed into, or brought into connection with a body of cold water, thereby becomes liquified, or reconverted into water; that, in fact, it is absolutely annihilated as steam, and that (as in the case of the steam-engine) the result of this annihilation necessarily would be the production of a vacuum in the cylinder.* We have here, then, to consider how far this theory is consistent with fact, and what are the grounds on which it is assumed to be connected with the vacuum.

It may be said that if this theory be fallacious, the great judgment and observation of Watt must have been at fault, and the scientific world, since his time, has been following an *ignis fatuus*—the condensation of steam by the direct action of cold water.

In addressing the French Institute, M. Arago, in his celebrated *Eloge* on Watt, and in reference to Mrs. Muirhead's reproof of the idleness of her nephew, the young Watt, in "dabbling with the tea kettle," observed: "In the year 1750 each one of us in the same situation as Mrs. Muirhead would have perhaps used the same language. But the world has made a stride, and our knowl-

* Professor Rankine says: "The ordinary condenser is a steam and air-tight vessel of any convenient shape, in which the steam discharged from the cylinder is liquified by a constant shower of cold water from the rose-headed injection valve."

edge has grown greater; and so when I will explain to you that the principal discovery of our fellow-member was a *particular mode of converting steam into water*, the little James Watt before the tea kettle becomes the mighty engineer, preluding to the discoveries which were to immortalize him; and it will by every one immediately be deemed worthy of remark, that the words '*condensation of steam*,' should naturally have to come to find a place in the history of Watt's childhood."

It would, however, appear that as our knowledge on this subject is still but imperfect, the limit of our stride is susceptible of a still larger advance. It certainly is not a desirable task to be any way instrumental in weakening so agreeable a reminiscence, or questioning any thing coming either from Arago or Watt.

In our search after truth, however, and in justification of science, as no respecter of persons, it becomes necessary not only to question the truth of this particular mode of "*converting steam into water*," but to characterize it as a misconception, if not an absolute fallacy.

Whatever, then, may be the merit of *the separate vessel*, which is in truth the great element of Watt's success, the theory, as regards *the action of cold water* in producing the vacuum, is altogether erroneous. The belief in this theory, which so universally prevails in our day, is perhaps not so much an impeachment of Watt's judgment as a reproach to the present age in the department of scientific research and experimental proof.

But let us examine Watt's own account of his discovery: "A boiler was constructed which showed by inspection the quantity of water evaporated in any given time, and thereby ascertained the quantity of steam used in every stroke of the engine, which I found to be several

times the full of the cylinder. Astonished at the quantity of water required for the injection, and the great heat it had acquired from the small quantity of water in the form of steam which had been used in filling the cylinder, and thinking I had made some mistake, the following experiment was made: A glass tube was bent at right angles; one end was inserted horizontally into the spout of a tea kettle, and the other part was immersed perpendicularly in well water, contained in a cylindrical glass vessel, and steam was made to pass through it *until it ceased to be condensed*, and the water in the glass vessel was becoming *nearly boiling hot*. The water in the glass vessel was then found to have gained an addition of about *one-sixth part* from the condensed steam. Consequently, water converted into steam can heat about *six times* its own weight of well water to 212°, or *till it can condense no more steam*." The inference here drawn, as to the condensation of the steam, may have been a *prima facie* and natural one, but that it was hasty and incorrect, and ultimately led to a long series of errors, will appear in the sequel. "Being struck," he continues, "with this remarkable fact, and not understanding the reason of it, I mentioned it to my friend, Dr. Black, who then explained to me his doctrine of *latent heat*, which he had taught for some time before this period (summer, 1764), but having myself been occupied with pursuits of business, if I had heard it, I had not attended to it, when I thus stumbled upon one of the material facts by which that beautiful theory is supported."

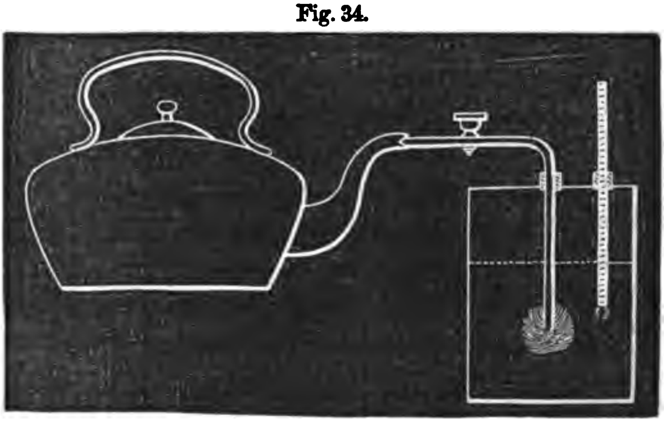
Through the whole of this experiment, it is manifest that Watt considered: 1st, that the steam was *bona fide* reconverted into water; 2d, that the heat, as explained by Dr. Black, was transferred to the water, and there retained "until it was become nearly boiling hot;" 3d, that this went

on until the steam "ceased to be condensed;" and, 4th, that this experiment illustrated and supported that "beautiful theory" of latent heat. On a closer examination, however, it will be found that Watt was deceived, as we all have been since his time, and that none of these assumed facts will bear the test of inquiry or proof.

If it were really true, that the heat was transferred to the water and retained by it while still in the liquid form, his inference would have been correct. In such case, however, the necessary result must have been that, with the loss of their heat, the vapor atoms would irrecoverably have lost their identity and characteristics as an elastic fluid; that mutual repulsion would have been changed to mutual attraction, and gravity then become the only moving power.

The following experiment, made many years ago, may here be referred to in disproof of such results. An apparatus was fitted in conformity with Watt's statement, and expressly to test his inferences. Four pounds of water at 60° were put into a kettle, and four pounds into a glass vessel with tubulures, as shown at Fig. 34. The kettle

Fig. 34.



was placed over an Argand gas burner, and made steam tight by wedges between the lid and the handle. The nozzle was connected by a tube with an intermediate tap. The steam was then raised in the kettle and passed into the glass vessel. A thermometer, passing through a steam-tight cork, being inserted in the water, on the temperature being raised in the glass vessel to 212° by the steam from the kettle, and when, as Watt supposed, the water would "*condense no more*," the tap was shut, and the water immediately poured out into a flat dish. Instead of finding a body of "*water nearly boiling hot*," there was a mixed body of water and steam—the latter rising in large clouds, and for a considerable time.*

The question then suggested itself, and it is strange it did not occur to Watt, where did this steam come from, if the heat had merely gone to make the water "nearly boiling hot?" What brought the steam so into connection with the water that it rose out of it as pure steam as it did out of the kettle itself? and why was it not condensed before being poured out? Not doubting the fact of condensation, these questions, at the time, created great doubts in reference to what really passed in the engine condenser, and led the writer to further inquiry. The logical inference would be, that as the steam rose out of the water as soon as allowed to escape, it must previously have been held imprisoned, as it were, in the water, since, without heat, water could not be converted into vapor, none, however, having been applied to the glass vessel. It was, at all

* The glass vessel should from the commencement be well covered with double blanket, or much heat will be lost during the process by *radiation*, and, consequently, the weight of water much increased. Experimenters in repeating this process will be surprised at the difficulty of obtaining accurate results.

events, clearly manifest that the steam had ***not been condensed or reconverted into water.***

To say the truth, the kettle experiment, as made by Watt, was altogether a slovenly one. 1st. No account was taken of the quantity of steam generated, and momentarily passing away from his open glass vessel during the process, or that rose after the temperature had reached 212°, and which is very considerable. 2d. No estimate was made of the heat radiated from the vessel during the operation, every unit of which must have left an atom of liquid matter behind. 3d. No thought was ever given to the quantity of liquid atoms or globules which we see pass from water, not only during ebullition, but long before the boiling temperature is reached, and which constitute what is called *priming.*

It is manifest, then, that Watt's calculations must have been greatly in excess when he says, "the water in the glass vessel was found to have gained an addition of about $\frac{1}{6}$ part from the condensed steam," seeing that so much of that $\frac{1}{6}$ had remained in the state of pure steam, ready to escape on being poured out, and when, as it were, a wider door or larger surface was thus opened for its exit.

No doubt the addition of $\frac{1}{6}$ was found in the glass vessel; this quantity would, however, be largely influenced by the time occupied in the experiment, and the rapidity of the process.

In truth, it appears, Watt never thought of looking for steam in any shape or place, imagining that the entire of that which had passed from the kettle had been *bona fide* reconverted into water.

On the above experiment being repeated, and the tap being shut, the water, both in the kettle and the glass vessel being allowed to cool to 60° and then weighed the

former had lost 12 ounces, and the latter had gained the same weight—that amount of water, in the state of vapor, having passed from the one vessel to the other.

On again making the experiment, the water from the glass vessel was poured into the flat pan, and the vapor allowed to escape. The result was, that instead of finding an excess of water, as before, equal to 12 ounces, the excess amounted to but 3½ ounces, as here shown:

	lbs.	oz.	lbs.	oz.
Put into kettle			4	0
Remained in after the experiment	3	3½	4	0
Passed into glass vessel as steam	0	12½		
Put into glass vessel	4	0	4	12½
Received from kettle as steam	0	12½		
Remained in pan after being cooled, by the escape of the steam	4	3½	4	12½
Weight of steam evaporated from the water in pan	0	9		

We thus see that out of the 12½ ounces which had left the kettle in the form of steam, only 3½ were found after having been poured out into the pan and allowed to evaporate and cool; consequently, 9 ounces was the weight of the steam which rose from the pan. Now, if loss of heat by radiation could have been prevented both from the glass and the iron pan, the entire 12½ ounces which had passed from the kettle would all have been recovered in the form of vapor, as were those 9 ounces. So in Watt's experiment, had there been no loss by radiation, the entire of the ⅙ part which had been gained in his cylinder vessel would all have risen in this state of steam, if allowed to do so, and no condensation would have taken place.

We see that the steam which Watt assumed to have been converted into hot water had been merely mixed—*diffused* among it, in the same way that air, carbonic acid gas, or other elastic fluid would (still, however, retaining its independent vaporous condition), until allowed to escape.

No doubt the abstraction of the steam from the cylinder naturally induced the belief that it had actually parted with its heat to the cold water, and, by condensation, had produced the desired vacuum. We have then, not only to test these assumed results, but to investigate and determine what really did take place on the steam and the cold water being brought into contact, and what was the cause of the vacuum indicated by the barometer.

In the first place the rush of the steam into the condenser, by virtue of its expansive property, would, as described by Watt, at once reduce the quantity in the cylinder by one-half, supposing they were of equal capacities. As, however, the usual proportions are for the condenser to be but half the size of the cylinder, the reduction of volume would only be one-fourth of that in the cylinder.

2d. The cold water, by the mode of its injection, being spread and dashed against the inner surface of the metallic condenser, the latter necessarily becoming cold, acts the part of a true surface condenser, in the same way as if the water had been made to act against the *outside* as in the still. Thus an absolute conversion of the steam into water is effected, and an absolute *pro tanto* vacuum produced, in proportion to the extent of the available surface, and its reduced temperature.

Had the inside surface been sufficiently extended, and cold enough, the entire of the steam would have been con-

densed, and a perfect vacuum formed. Practically, however, but a very moderate portion of the steam is so disposed of.

3d. The water ejected into the condenser becomes rapidly mixed with the incoming steam. So far, however, from the latter losing its heat or being condensed into the state of water, it is merely mechanically diffused, as already observed, on the true Daltonian theory, among it, in proportion to the respective quantities of each.

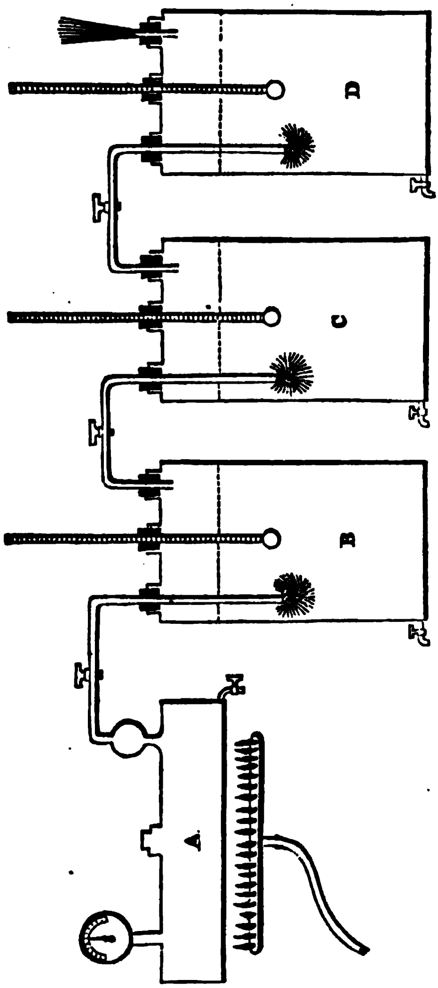
It may then be broadly and unequivocally stated that there is no other mode by which steam can be condensed —that is, reconverted into water—than by the abstraction of its heat by metallic refrigeration, as is done in the still, or in Hall's system of metallic tubes. That in a word there is none other than *Surface Condensation.*

That water, whatever may be its temperature, is incapable of reducing steam to its previous state and bulk of water, is susceptible of direct proof, and may thus be shown. In Fig. 35, *A*, an iron boiler capable of holding eight pounds of water, was fitted with a Bourdon pressure gauge, and heated by the flame of gas from an iron tube having a double row of small orifices for the issue of the gas. *B*, *C*, and *D*, are copper vessels furnished with tubulures, and the necessary intermediate taps to separate their respective contents, and prevent the return of the water on cooling, by which the tubes would otherwise act the part of syphons. (These two taps should be shut before that which is in connection with the boiler.)

Each copper being furnished with a thermometer inserted in an air-tight cork, four pounds of water at 60° were put into each. The gas being lighted under the boiler, so soon as the water in it was saturated, the steam

began to pass into the copper *B*, and in twenty minutes the thermometer indicated 212°. In the same way, on being saturated with steam, it passed into *C*, and then into *D*, from which it issued in full force.

Fig. 35.



Now, if steam could have been annihilated, or converted into the liquid form by the agency of cold water, it would have taken place in the first vessel *B*. Nothing of the kind, however, occurred. So soon as that vessel obtained its saturating quantity, it passed into the cold water in the second, and so on to the third, the cold water in each merely acting the part of a vacuum or medium for the reception of the steam.

On the third vessel becoming thus saturated, and the thermometer indicating 212°, the intermediate taps were shut, and the water let out into separate flat dishes, the steam rising from each as

already described. When all the steam had escaped, and the temperatures had returned to 60°, each was again weighed, and the usual amount of escape by evaporation was ascertained.

As copper *B*, however, had been longer under the operation than *C*, the weight of water in excess of saturation, caused by radiation, was necessarily found to be greater in *B* than in *C;* and so in *C* than in *D*.

In truth, had there been one hundred such vessels, the steam would have passed through the water in all, and not a single atom would have lost its heat to the water, or been condensed. If cold water, however, had been poured on to any of these copper vessels, they would become absolute metallic refrigerators, and a vacuum would instantaneously have been effected in it.

It is remarkable that none of the numerous writers on this subject have gone into this question of the assumed effect of water in depriving steam of its heat, and reconverting it into the liquid state. It appears to have been adopted by all as a truism, and a natural, though merely supposititious inference, from the successful use of the separate vessel—the condenser. Nevertheless, such a theory is incompatible with the patent and indisputable fact of the steam rising in such volumes from the so-called condensed or heated water on its being poured out. When a solid body is heated and allowed to cool, the heat is radiated from it. But none of the *matter itself* passes away, and, consequently, no weight is lost. When, however, a body of so-called heated water is allowed to cool (unless confined in a close vessel), not only does this heat escape by radiation from the vessel, but a portion of the *water itself*, in the form of *steam*, passes away, and weight is lost. From this we would infer, not that the heat was really combined

with the water, in the *liquid form*, in which case it would have passed off by mere radiation, but that it remained associated with water, in the form of vapor, the atoms of which, by reason of their levity, rose out of the water, and carried away each its constituent heat along with it. The subject manifestly requires further investigation.

So long, then, as we remain ignorant of the true reciprocal relations between vaporous and liquid atoms, under the operation of an increased or diminished quantity of heat, we must remain alike incapable of deciding on the various results of temperature, pressure, and volume. The right understanding of the process carried on in the separate condenser is therefore absolutely essential to our deciding on the most effective mode of producing a good and available vacuum in the cylinder.

Water, however, having no direct action or influence in producing the condensation of the steam, or taking up its heat, undivided attention must then be given to the means of improving the effect of *metallic refrigeration.* This may be done by enlarging the area of heat-absorbing surface, or increasing the rapidity of its action. Suggestions on these points will hereafter be offered.

SECTION X.

ON EVAPORATION.

It has been well observed that "accuracy in the use of language implies accuracy of reasoning." The reverse may be applied with equal truth, and in no instance, in the range of philosophical inquiry, is that reverse more applicable than in the branch of the subject now before us. The absence of accuracy is also here the more remarkable, seeing its recognized importance in all chemical investigations and analyses under the nomenclature.

The formation of *vapor* atoms from *liquid* atoms has been already described, and we have now to consider the conditions under which *the former are separated, and escape from the water.*

The distinction between the *generation* of vapor, and its mere *escape* into the air, would appear so self-evident that it is the more extraordinary there should be any doubt on the subject, or any room for inaccuracy in describing their respective peculiarities.

Nothing, however, is more common, even among writers of the highest authority, than to find the terms *vaporization* and *evaporation*, not merely confounded, and used as if they were synonymous, but actually *reversed*, and thus so misplaced as to lead to serious practical errors. Notwithstanding the indiscriminate and often utterly incorrect use of these terms, no two processes in nature can be more distinct. *Vaporization*, as already observed, being the conversion of liquid atoms into those of vapor by the absorption of heat. *Evaporation*, on the other

hand, being the mere escape of the vapor atoms *so formed*, and without reference to heat. This distinction then is neither empirical nor arbitrary, but chemically, philologically, and *vi termini*, representative of the processes themselves. A few instances may here be given of their misapplication.

"*Evaporation*," says the *Encyclopædia Britannica*, "in natural philosophy, is that process by which water and other liquids are *converted into steam*, an elastic fluid, and dissipated in the atmosphere." This description is as inaccurate as it is explicit. Water, or other liquids, assuredly are not *converted into steam* by evaporation. The cause and effect are here confounded. The term evaporation has no meaning but as expressing the departure or removal of vapor, as when speaking of emigration, there can be no departure until there be an emigrant, or somebody to emigrate. So there can be no evaporation until there be vapor to escape. With equal correctness and propriety might it be said that the process of generating the gas from the coal in the retorts, by the heat with which they are surrounded, is the same as that by which they pass through the pipes and escape from the numerous orifices in our street burners. In both cases, the *generation* and the *escape* are equally distinct processes. Evaporation, then, is not concurrent with, but consequent on the *previous* act of vaporization. The neglect of this distinction involves the error of implying that liquid atoms are converted into vapor *only when they rise* and escape into the air; or, the equally erroneous inference, that watery atoms rise, *mero motu*, and, *as such*, are dissipated in the atmosphere.

Rees' *Cyclopædia* thus makes a similar mistake, and in the same words:—"*Evaporation* is that process by which water and other liquids are *converted into vapor* or steam,

an elastic fluid, and dissipated into the atmosphere." Now, if this description of evaporation were true, what meaning should be attributed to the term *vaporization?*

Again, as vapor is continually rising from water at all temperatures, and dissipated in the atmosphere, such an explanation becomes still more unintelligible and misleading, as it would ignore the use or action of heat altogether.

The *Cyclopædia* goes on to quote the opinions of philosophers which are here more entitled to notice:—"Aristotle ascribed the *formation of vapor* to the action of fire. Halley supposes small spheres of water to be filled with subtle fluid, so as to make them lighter than air. Desaguliers asserts that water is capable of being converted by heat into an elastic fluid much lighter than air." All these authorities are right in considering the *formation of vapor* to be the union of liquid particles with heat, but furnish no authority for the supposition that *evaporation* means the *imparting* of that heat.

Saussure says:—"*Evaporation*, properly speaking, is the result, or, rather, *effect* of the intimate *union of elementary* fire (heat) *with water*. By this union the water and fire combined form an elastic fluid, specifically lighter than air, and which is peculiarly distinguished by the name of *vapor*." Here we have a clear and correct description, not of *evaporation*, but of *vaporization*, and it is only when the vapor thus formed escapes that *evaporation* begins.

It is admitted by all, that "evaporation produces cold." This is correct, since, as increased temperature is derivable from the increased quantity of vapor atoms present, so the escape of these atoms is tantamount to the escape of the heat which they (each) carried away, and by which the sensation of cold was produced.

Rees' *Cyclopædia* says—"Cold is produced when any

part of the human body is moistened with water, and the same is suffered to evaporate." Here again is a direct case of *vaporization*—seeing that the atoms of such moisture must first be vaporized before they can pass into the atmosphere.

The following simple experiment will illustrate the two processes of vaporization and subsequent evaporation. Hold a champagne glass in the hand, as in Fig. 36, and

Fig. 36.

then pour into it some cold water—the colder the better. A sensation of cold is immediately experienced, indicating the loss of heat from the hand. The question then is, what becomes of that heat? The ordinary theory would say, that it had gone to heat the water. This is an error. It has gone, first to heat the glass, and then, not to heat the water, but to vaporize, or convert the atoms of the water, in immediate contact with the glass, into vapor,

each atom of the liquid receiving its unit of heat, and so becoming one of vapor.

What proof, it may be asked, have we of such conversion? The most tangible and incontrovertible, namely, the escape of that vapor and its becoming visibly condensed on a glass, or mirror laid on it, as observed at page 44, Fig. 3. Here then we have—1st. the vaporization of a portion of the water; and, 2ndly, the escape of that vapor, and its visible condensation.

In the *Philosophical Transactions* for 1792, in an essay on evaporation, by De Luc, it is said, "that vapor *in the air* is precisely the same as vapor *in vacuo*," and that there is "a certain minimum distance of the particles for each temperature at which they sustain their elasticity." This is correct, and in truth, involves the whole case. Dalton also observes:—"Steam in vacuo and steam in the air are precisely of *the same force at the same temperature*, forming a mechanical mixture, and not a chemical compound." Now, what is this but saying that similar quantities of vapor, wherever they may be found, will necessarily produce the same temperature and effect. This he further illustrates by stating that "the *quantity and force* of steam become synonymous terms."

It is here then insisted that this doctrine is equally true of vapor, whether it be in *water* or in the *air*, thus carrying Dalton's sound principles to their legitimate issue.

The *Cyclopædia* goes on to say, that, "though the reasons assigned by De Luc and Dalton for the non-condensation of steam already existing in the air, appear to be incontrovertible, yet no sufficient reason has been given by either of them for the *entrance of steam of low temperature* into the atmosphere. How does steam of a part of the force of the atmosphere at first penetrate it?"

The difficulty here unnecessarily raised is manifestly the result of regarding the steam as a body, or *en masse.*

It is, however, not the *mass* that "finds an entrance into the atmosphere," but it is several atoms, each of which, in its individual capacity, rises from the water, and, being lighter than the atoms of the air, ascends in it, as a balloon would from the ground. There can, in fact, be no such thing as a high or low temperature in an atom of steam—these terms solely applying to any difference of *quantity* in a body of water or air, each atom individually having its own specific gravity, volume, weight, and temperature.

Regnault falls into the same error, in mistaking vaporization for evaporation. "Water," he says, "easily takes the gaseous state; the temperature at which this change of state takes place depends on the pressure of the air."*

This is palpably erroneous. The *formation* of vapor is wholly independent of pressure. Had he said that the *escape* of the vapor from the water, *after it had been formed*, is influenced by the pressure of the air, he would have been correct. Yet the view taken by him is that adopted by most writers, and further, that the *generation* of vapor only takes place at the boiling point, many instances of which have been already quoted.

Now, had a true distinction been drawn between the two processes of *generating vapor* and its subsequent *escape*—in other words, between vaporization and evaporation—these errors could not have existed, as it must have led to

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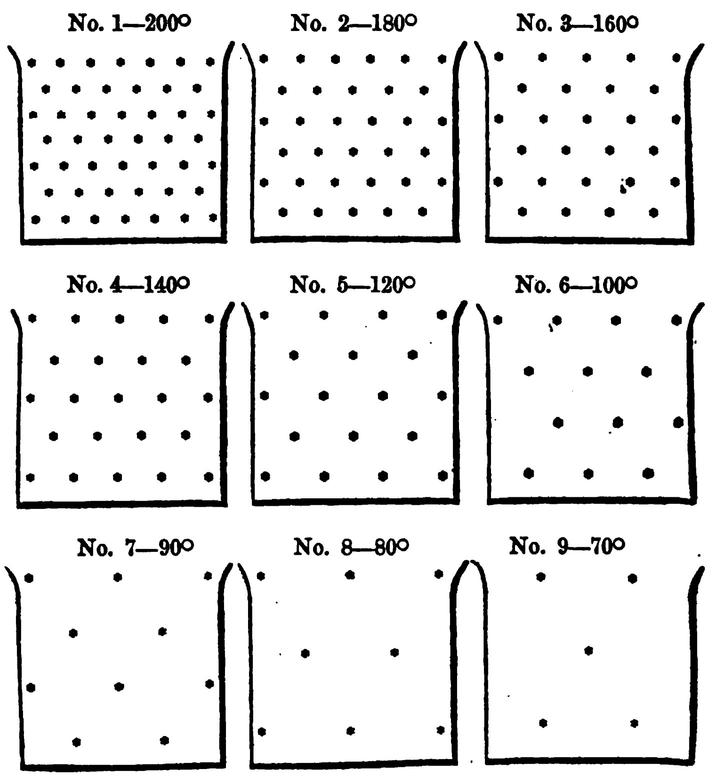
* "L'eau prend facilement l'etat gazeux; la temperature à la quelle ce changement d'etat a lieu depend de la pression de l'air. On a pris pour second point fixe du thermomètre, lequel est marqué 100 dans la division centigrade, la temperature à la quelle l'eau bout sous la pression de 760 millimètres de mercure. La temperature à la quelle cette ebullition a lieu diminue avec la pression, ainsi, l'eau bout sous une couche de glace dans le vide de la machine pneumatique."

a right understanding of the connection which heat bears to the liquid atoms, and have solved the question as to where, and how, and when the vapor was formed. Had the term and act of vaporization been correctly defined, so serious an error never could have existed as the associating the generation of vapor with the boiling point, or 212°, as Regnault has here done.

That reduction of temperature is merely the result of the escape of vapor will be satisfactorily illustrated by the following experiment: Let the nine squares, Fig. 37, be assumed to represent so many stages through which a

Fig. 37.



body of water in an open vessel passes under the process of evaporation, each stage representing the quantities of vapor remaining, with its state of diffusion, and consequent temperature.

In this we have a correct representation of Dalton's theory of diffusion. Let No. 1 represent the state and quantity of vapor existing in the vessel at the temperature, say, of 200°. As evaporation proceeds, and the quantity of vapor is reduced, so, necessarily, will be the reduced weight and temperature. The continuous process of diffusion, always producing a homogeneity of vapor, and consequently of temperature, in the mass, may here be visibly noticed—the stars representing the vapor in the water under the influence of diffusion.

Thus: No.	1	will represent	200°
	2	"	180°
	3	"	160°
	4	"	140°
	5	"	120°
	6	"	100°
	7	"	90°
	8	"	80°
	9	"	70°

"Dalton," says Thomson, "made a set of experiments to determine the rate at which water evaporates from a vessel of a given diameter, at different temperatures, supposing the atmosphere in which the evaporation is going on to be quite free from vapor." The following experiments will enable us to determine that question on a larger scale, and with sufficient accuracy to warrant the assertion that, although the times vary, the loss of weight, by evap-

oration, *cæteris paribus*, will always be commensurate with the loss of heat.

Four pounds of boiling water were poured into a flat circular pan, 12 inches diameter and 3 inches deep, placed on the scale of an accurately-balanced beam, with a thermometer suspended in the water. On the thermometer indicating 200°, the gradual reduction in *weight*, from the escape of the vapor and the time occupied, were noted at each reduction of ten degrees of *heat*. The loss of vapor was ascertained by weights, each being one-tenth of an ounce. The result was as follows:

Temperature in the Water.	Loss of Weight for every 10 degrees of Heat.	Time employed.
200°	In tenths of an ounce.	
190°	6 "	2 minutes.
180°	6 "	3 "
170°	6 "	4 "
160°	6 "	6 "
150°	6 "	10 "
140°	6 "	14 "
130°	5½ "	19 "
120°	5 "	26 "
110°	4½ "	34 "
100°	4½ "	47 "
90°	4¼ "	68 "

Here we see the escape of the vapor was accompanied by a reduced temperature and a corresponding reduced weight. Could radiation from the sides and bottom of the tin vessel have been prevented, the loss of each ten degrees of temperature would throughout have been accompanied by the loss of a uniform and commensurate weight.

The following further experiment may here be mentioned as exhibiting the two distinct processes of vaporization and evaporation going on at the same time, and in disproof of the received notion of vapor being only formed at the temperature of ebullition.

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Six pounds of water at 70° in the same pan were suspended over a large gas burner, and the temperature raised to 200°—the loss by the escape of the portion of the newly-formed vapor being noted for each increase of 10 degrees of heat. The heat was then withdrawn, and the observations continued until the thermometer had returned to its initial 70°. In this case the pan was cased in a jacket of india-rubber, with a view of retarding radiation. The effect, however, was rather injurious as soon as the temperature became reduced. A current of air was also continuously thrown on the surface of the water, by which the rate of evaporation was increased. The following table will give the results. It will there be seen that, although for some time the rate of evaporation continued uniform with the loss of heat, still, as before, so soon as the jacket became cold and radiation became considerable, the discrepancy between the loss of heat and weight became considerable, heat being then rather given out to the jacket.

Temperature in the water.	Loss of weight for every ten degrees of heat.	Time occupied in minutes.	
70°	Tenths of an ounce.		
80°	1 " "	2 minutes.	
90°	1 " "	3½ "	
100°	1 " "	4 "	
110°	1 " "	5½ "	
120°	1 " "	7 "	
130°	1 " "	8 "	ounces
140°	1½ " "	10 "	6·9 evaporated.
150°	2 " "	12 "	
160°	3½ " "	14 "	
170°	5½ " "	16 "	
180°	10 " "	19 "	
190°	16 " "	22 "	
200°	25 " "	27 "	
	HEAT WITHDRAWN.		
190°	8 tenths of an ounce.	1 minute.	
180°	8 " "	1 "	
170°	8 " "	2 "	
160°	7½ " "	3 "	
150°	7 " "	4 "	
140°	6½ " "	4 "	ounces
130°	6 " "	4 "	7·7 evaporated.
120°	5 " "	6 "	
110°	4½ " "	9 "	
100°	4½ " "	14 "	
90°	4½ " "	20 "	
80°	4 " "	26 "	
70°	4 " "	30 "	

The inferences from these results are—

1st. That the vapor which escaped unremittingly into the air, under the process of evaporation, must have previously existed *in the water*—its escape being proportioned to the area of the surface exposed to the lighter medium of the air.

2d. That the reduced temperature was the concomitant of the reduced weight of vapor escaping from the water.

3d. That the increasing time required for the escape of each quantity of vapor was clearly accounted for by the *reduced quantity* or number of atoms momentarily and successfully exposed to the air in the upper stratum of the liquid under the operation of diffusion in the mass.

Let us examine this process of evaporation still closer, and in reference to the surface area. Let Fig. 38 represent an open vessel containing one pound of water at a temperature of 200°, and having a surface of *one square foot;* and Fig. 39 another vessel containing the same

Fig. 38.

Fig. 39.

quantity of water, at the same temperature, but with an exposed surface of *two square feet.** As the escape of the vapor takes place from the surface, where it is in direct contact with the air above it, it is manifest that, as the surface area is double in the latter case, so the number of vapor atoms in the upper stratum, as here shown, will be double, and consequently the rate of evaporation, or escape

* The circles in the centre represent the atoms of vapor; the radii represent the diverging or repulsive influence of each.

of the vapor, will also be double, in equal times. Diminished temperature, then, is not the *cause*, but the *consequence* of a diminished quantity of vapor present in a body of water. What is then meant by *cooling* is neither more nor less than either allowing its vapor to escape into the air, as from an open vessel, or, by its loss of heat from radiation, by which the vapor becomes reduced to its previous state of liquid.

Thomson says: "When the air is perfectly still, the vapor, "*as it forms*, accumulates over the surface of the water." Here the *formation* of the vapor is assumed to be contemporaneous with its escape, despite the common-sense view that the vapor must have been *formed* before it could escape, except on the absurd supposition that it was produced by contact with the cold air above it.

Among the authorities consulted on the subject of this treatise, was one which cannot here be omitted, inasmuch as it is presented, somewhat ostentatiously, "for the use of the officers of Her Majesty's navy;"* and the more so, as it furnishes conclusive evidence of the necessity of further inquiry, and of the still unsettled state of the several questions considered in these pages. It is to be remarked, that in this volume the all-important subject of the existence of vapor in water, and which is so directly connected with the disputed cause of explosions, is not even alluded to. Further, in a work professing, among other points, to be an inquiry into the subject of heat in connection with water, and all that appertains to the generation of steam, and its use in the steam-engine, the very

* "The Marine Steam-Engine, designed chiefly for the use of the Officers of Her Majesty's Navy," by Thomas J. Main, M.A., F.R.Ast.S., Mathematical Professor at the Royal Naval College, Portsmouth, and Thomas Brown, Chief Engineer, R.N., attached to the R.N. College.

heads, *Vaporization* and *Ebullition*, are not to be found in an otherwise comprehensive table of contents, embracing no fewer than 370 articles.

There is, however, one article on *Vapor and Steam*. Naturally looking to that for a copious and elementary view on the subject, the whole is found to be compressed into six lines, and these containing as many errors. The following is the article *in extenso* (see page 26, article 37).

"To distinguish vapor from steam, vapor is formed only at the *surface;* steam from the *body* of the liquid. Evaporation proceeds at *all* temperatures. Steam is formed when the fluid has arrived at a *certain fixed* temperature. The formation of steam is a *violent* process; the formation of vapor is *gradual* and *insensible.*"

It is much to be regretted that it becomes necessary to point at least to the absence of inquiry on the subject, which involves the due management and working of the boilers, as regards the generation of steam, and those essentials which involve the cause and prevention of explosion; of which we have lately had so fatal an example in the Great Eastern.

This article may then be thus summarily reviewed. The following are among the statements it contains—viz.:

1. A distinction is drawn between *vapor and steam.*
2. "Vapor is *formed* only at the *surface.*"
3. "Steam is *formed* from the *body of the fluid.*"
4. "Steam is *formed* when the fluid has arrived at a *certain* temperature."
5. "The formation of steam is a *violent process.*"

Whatever may be the merits of the work in other respects, it is only necessary, as regards the above, to say, that each and every one of the above statements is theoretically and practically erroneous.

SECTION XI.

OF SPONTANEOUS EVAPORATION.

So MANY writers of high standing have in their works devoted each a separate section and consideration to the branch of the subject embraced under the above head, that it is here considered advisable to adopt a similar proceeding.

Among the anomalies resulting from the want of a correct knowledge of the relation which heat bears to water in its liquid and vaporous states, none are more remarkable than those connected with the process of evaporation. Unable to reconcile or account for the fact of the escape of vapor at all temperatures, in the absence of any reliable explanation of place or means by which the vapor is formed, writers have recourse to the theory of "*spontaneous evaporation.*"

"Water slowly evaporates (says Professor Brande) *under exposure to the air;* its vapor mixes with the surrounding atmosphere, and the process is usually called *spontaneous evaporation;* it takes place at all temperatures, and with a rapidity proportionate to the dryness of the air, and the velocity of the current passing over it."

There is here an oversight of importance which merits attention. The evaporation from water is here assumed to be in consequence of its "*exposure to the air.*" This is not the case; the evaporation, or escape of the vapor, is always going on, whether the water be so exposed or confined in a vessel apart from the atmosphere. If we half fill a large bottle, as shown at page 141, Fig. 30, and tightly cork it, we find the vapor continuously rising from

the water, and being condensed on the *upper* part of the glass always trickling down, and again returning into the water, so that no loss of weight is sustained; while the *lower* part of the bottle, receiving heat from the temperature of the room, produces a continuous supply of new vapor in the water, as described at page 188, Fig, 36.

Now nothing can be more unphilosophical or embarrassing than this supposed endowment of the liquid with a species of moral aptitude or will. If it were physically true that *water* evaporated or separated itself from the mass, it would imply that the liquid atoms, by some impulse or inherent power of their own, were converted into vapor; or that, notwithstanding their individual weight and mutual attraction, they separated from the mass, and rose into the air.

The term *spontaneous* seems to have been adopted as an excuse, or *quasi* reason, by writers unable to assign a sufficient cause for the diminution of the mass of water exposed to atmospheric influence. It is, however, an unmeaning and inapplicable term when applied to brute matter. "*Spontaneous*," as our lexicographers say, means "voluntary, acting of itself, implying a will which may or may not be exercised, and a power of acting of its own accord." Such, however, is wholly inapplicable in reference to physical laws. With equal propriety may it be said that a cork from the bottom of water, or a balloon from the earth's surface, rises by some spontaneous impulse. To speak of water being heated or expanded, may be a convenient mode of expressing a convenient theory; the idea, however, of *spontaneous evaporation* is so utterly at variance with the laws of inorganic matter, that the term never should have had a *locus standi* in an inquiry of a philosophical or chemical character.

But, in truth, there is no necessity for this recourse to a supposed spontaneous action. All the movements in connection with evaporation are so subject to the immutable and well-known laws of gravitation and diffusion, and now so intelligible, not to say visible, that it is a matter of equal surprise and regret that it should have been so pressed into the service. If we examine the subject carefully and practically, we shall find that the rise and escape of vapor atoms (which virtually is evaporation) is as much under the influence of gravity as that of the cork in water. By the ordinary theory, however, we are left to infer that vapor atoms only have an existence *as they rise from the surface* of the water, and, consequently, are then formed without any further accession of heat.

Professor Graham (vol. I. 268) says—"in the process of evaporation the vapor is supplied only from the superficial layer of the liquid." This leaves the all-important questions undecided, namely, where was that vapor formed, where did the heat come from by which the continuous supply of vapor was produced, and by what means, and where, did it obtain those properties which he admits are the distinguishing characteristics of vapor? It would, then, have been more to the point to say, the supply came from the *superficial layer, or stratum of vapor atoms*, in the water, as already described. (See back, page 42, Figs. 1 and 2.)

The Professor then goes on to say—"It is, therefore, evident that extent of surface must greatly influence the amount and rapidity of evaporation." No doubt of that. Extent of surface, however, can have no influence on the *formation* of the vapor, although it has on the amount of evaporation—that is, on the quantity escaping in given times. The difficulty of obtaining an intelligible view of

the subject is further increased when he says—"Vapor, though slowly *formed* in cases of *spontaneous evaporation,* is similarly constituted to that produced by rapid ebullition."

Turner also says—"Evaporation, as well as ebullition, consists in the *formation of vapor*, and the only assignable difference between them is, that the one takes place *quietly*, the other with the appearance of boiling."

Nothing can be more unwarranted than this view of the subject. Here, vaporization, or the "*formation of vapor*," is clearly mistaken for evaporation, or its escape; and as to *ebullition*, it has already been demonstrated that it has no relation whatever to such formation.

"Scientific men (he goes on to say) have differed concerning the cause of evaporation. It was once supposed to be owing to chemical attraction between the water and the air. The experiments of Dalton prove that *heat is the true and only cause of the formation of vapor.*" How, then, can this undoubted truth be reconciled with the theory of the vapor being "supplied only from the superficial layer of the liquid," *where no such heat could be applied?*

Again, Turner adds—"Water, placed under the receiver of an air-pump, evaporates with great rapidity." So, also, Regnault says—"l'eau *bout* sous une couche de glace dans le vide de la machine pneumatique." These opinions, then, only make the confusion more palpable, as no heat could possibly be supplied, to *form vapor*, under such circumstances.

In proof of the continued adoption of the same theories, we find it stated in a late work, that "water, which is liquid under the ordinary pressure and temperature of the atmosphere, *assumes the gaseous state*, either by *diminishing the pressure*, or by increasing the temperature in a

certain ratio.* Under the atmospheric pressure, and at a temperature of 212°, it *becomes gaseous*, or *evaporates*." Here, the becoming gaseous, and evaporating, are used as synonymous terms and representing similar processes. How liquids can be converted into the gaseous form by merely diminishing the pressure is inexplicable; and that vapor is formed only when at 212°, is refuted by its appearance and condensation, as before shown, from the moment the heat is applied.

There can be no wonder, then, that "scientific men should differ concerning the cause of evaporation," seeing that they begin by assuming that the heat applied, either from above or below, goes to *heat the water*, rather than to *convert it into vapor;* and that vapor has its existence only as it rises out of the water, or when at the boiling temperature.

We see, then, how inextricably involved is the subject under the prevailing theory, and how impossible it is to reconcile the anomalies which present themselves at every stage of the inquiry.

It may here be noted, that while the authority of Dalton, on the diffusive property of vapors, is recognized by all writers, they ignore its effect practically, both in vaporization and evaporation. Nevertheless, all these anomalies and contradictions are at once reconciled by the application of those very laws which Dalton has so clearly explained.

Sir R. Kane repeats the assumed theory of *spontaneous evaporation* thus:—"The *conversion of a liquid into vapor*, at *ordinary temperatures*, is often called *spontaneous* evaporation; and in the case of water, from the great extent to

* "The Steam-Engine for Practical Men." By James Hann, A.I.C.E., Mathematical Master of King's College School, and Justo Gener. Civil Engineers.

which it becomes subservient to the economy of nature, the process is one of high importance."

A distinction is here sought to be inferred between evaporation under "ordinary temperatures," and that which may be produced under other circumstances. This is, however, untenable, as all authorities admit that vapor is the same whatever may be the temperature under which it is generated. In the above, the *conversion of a liquid into vapor* is manifestly confounded with the escape of the vapor so formed.

Thomson, also, has devoted a separate section to the consideration of "*spontaneous evaporation*," beginning thus:—"Everybody knows that water evaporates at all temperatures, however low." Here we have a palpable misconception, arising out of the inaccurate use of terms, enunciated as an axiom or self-evident proposition. Here we have the *petitio principii* at issue, which, if admitted, would put an end to further inquiry. Now, everybody knows that *it is so said;* none, however, have proved it. Vapor escaping from water is certainly no proof that the *water* evaporates or escapes. The evaporation *of water*, and the evaporation *from water*, would be distinct processes, and should have distinct meanings. In truth, the opinions of scientific men, as to the cause or process of this escape of vapor—this so-called *spontaneous evaporation*, are as variable as if nobody knew any thing about it.

"Let us consider (he observes) the evaporation of water, at *low temperature*, with attention. The evaporation is entirely confined to the *surface*, and is therefore in proportion to that surface." It might be here asked—Where else could the vapor have had the means of escape but at the surface, the only door open for its departure? As well might it be gravely stated that the escape of a prisoner

from gaol, or of a bird from a cage, will be confined to the open door, seeing that they could no more escape through the bars than vapor could through the glass sides of the vessel.

With reference to the alleged "accumulation of vapor over the surface of the water, and that in proportion as this accumulation increases, does the process of evaporation diminish." This is seriously open to doubt, seeing that as rapidly as the vapor rises it is diffused through the atmospheric mass, which, therefore, can never be surcharged directly over the water.

The true cause of the diminution *in quantity* of vapor escaping in *given times*, it will be seen, consists in the diminishing quantity contained in each successive surface stratum in the water, as already explained.

When we say that "*water evaporates*," the expression is absolutely incorrect, unless we add—"*in the form and state of vapor*." This, however, involves the very change and question in dispute.

It is, then, not atoms of *liquid*, but those of *vapor*, that go off: thus still raising the question—Where was that vapor before its escape, and how was it formed?

If the distinction between liquid and vapor atoms were merely verbal or unimportant, the incorrectness in the use of the terms would be equally so.

When, however, it practically involves the difference between attraction and diffusion—between the *vis viva* of repulsion, and the *vis inertiæ* of quiescence, and the difference between the states of positive and negative electricity, the error in terms becomes important in its results, and hence the confusion arising out of the conflicting theories which prevail. Had the subject been rightly examined, we never could have had those imaginary distinctions and

involved hypotheses which we see enumerated by Pambour, and many others.

In the illustration of his views of "*water evaporating*" Thompson observes:—"Sea salt is obtained by allowing sea water to evaporate *spontaneously*, till the salt separates in crystals." With equal truth might it be said that the salt is obtained by allowing the brine in the pans in Cheshire to evaporate *spontaneously;* or in the distilling apparatus used in our steam vessels for obtaining fresh water; for it must be a matter of indifference, as regards the process, whether the heat which produced the formation of the vapor comes from the *sun above*, or from *flame* beneath, as shown in section on Vaporization. See back, page 42, figs. 1 and 2.

For the purpose then of accuracy in terms, as conducive to accuracy in reasoning, it would be well if experimenters would determine the true value of the terms, and with a more definite nomenclature give the world a correct explanation of the difference between vaporization and evaporation. If the question is to remain in its present unsatisfactory state we may at once admit the two terms to be synonymous, and abandon the idea of heat being necessary to the formation of vapor, which, otherwise, is incompatible with the idea of evaporation going on at all times and temperatures, as Thomson observes, when he says:—"Ice and snow are constantly emitting vapor, and diminishing in weight when surrounded by a dry atmosphere." The Cyclopædia further adds:—"The spontaneous evaporation of water, or that which is constantly going on from all parts of the surface of the globe, has always been an interesting phenomenon to the speculative philosopher." We have in fact too much of this speculation going on, while a few physical facts, illustrative of Dalton's principle,

puts the whole in so intelligible a form as to reduce it to the simplest and most evident illustration.

If then we look at vapor as the mere assemblage of individual atoms, as we do those of any other elastic fluid or gas, each atom possessing its due equivalent of heat, and exercising its properties of force, repulsion, and pressure, in the ratio of number or quantity, and the space occupied, independent of the medium in which they may be placed, there can be no room for speculation.

With reference to the continued process of evaporation, meaning the unremitting escape of vapor atoms from water, it is only necessary to consider, that vapor atoms, individually, being specifically lighter than those of water or air, must always be under the influence of their property of buoyancy, whatever may be the medium in which they may happen to be. In proof of this we have only to place an open vessel on the scale of a well-adjusted beam, and it will be found continuously, night and day, to be reduced in weight by the escape of its vapor—the *heat* continuously passing away with the vapor, and being as continuously restored, and new atoms of vapor formed, if the surrounding temperature be a single degree in advance. Thus the temperature, in the water, will always be sustained, and evaporation always going on: for, although the weight of a body of water will be continuously reduced by the escape of the vapor, the temperature will continue equal to that of the surrounding air.

If the temperature be about 65°, and the surface of the water be that of a common saucer, the loss of weight will be about three to five grains every hour. Should the temperature of the room be increased, the quantity evaporated will, of course, be increased, because more vapor

will continuously be formed in the water, and each stratum will, consequently, contain a greater number of atoms.

Professor Graham also devotes a special paper (vol. I. page 90) to "the diffusion of vapor into the air, or spontaneous evaporation."

On this he observes, "We are indebted to Dalton for the discovery that the evaporation of water has the same limit *in air*, as in a vacuum. Indeed the quantity of vapor which can rise into a confined space is the same, whether that space be a vacuum, or be already filled with air or gas in any state of condensation. Hence, it is only necessary to know what quantity of vapor rises into a vacuum at any particular temperature—the same quantity rises into the air."

It has already (see page 55) been observed, that according to Dalton, "gases *mixed with water* retain their elasticity or repulsive power amongst their own particles, just the same in water as out of it—the intervening water having no other influence, in this respect, than a *mere vacuum*."

How then can there remain any doubt either as to the existence of the vapor, an elastic fluid, in the water, or its escape out of it into the air, at all temperatures? The whole difficulty arises from considering the rise of the vapor from the surface of the water, *apart from* its previous existence in it. Of the fact of the vapor rising out of the water there can be no more conclusive evidence than the physical test of its condensation. In this there can be no deception—our senses furnish absolute proof. Fill an open glass vessel with water, irrespective of its temperature, and lay it on a dial or glass cup with some cold water in it (see page 47, fig. 6), and it will always exhibit by condensation, the vapor rising out of the water.

We might here close the inquiry into the existing theories. We have, however, one other, and which, as being the most recent and unquestionably one of the highest authority, merits particular attention, although it will be found to present but a repetition of the same views, and, unfortunately, to leave the question in the same uncertainty.

In a recent number of the new *Encyclopædia Britannica*, in the article on Meteorology, by Sir John Herschel, there are some remarks on the subject of evaporation which here demand notice and a rigid inquiry. "Water," he observes, "exposed to the air, evaporates at all temperatures." This leaves the question in doubt, as to what it is that goes off. Is it atoms of *vapor* or of the *liquid*—water? It cannot be the *latter*, since, by their superior gravity, they could not rise into the air. It must then be the *former*, and this involves the very point at issue, namely—the presence of already-formed vapor in the water.

If this be not admitted, it then assumes the necessity of their formation by, and on their separation from, the liquid, and *without further heat*.

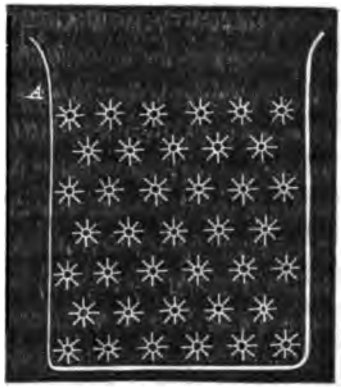
Again: "If the air be already moist, evaporation is proportionably retarded, the force of the escape being the difference between the elastic tensions of the *generated vapor* and of that already existing in the air." With the highest respect for this authority, it must be said that there is great obscurity in this passage.

Here the escaping matter is distinctly called "*generated vapor*," yet we are left in doubt as to *where* the generation has taken place, and thus still embarrassed between the two theories; either of the *previous existence* of this vapor in the water, which, as Dalton states, acts the part of a

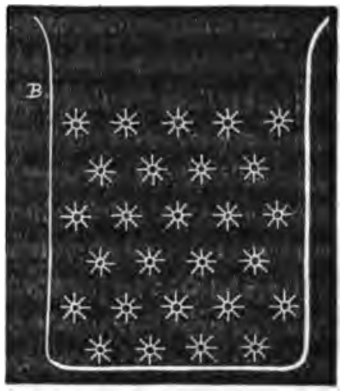
vacuum, or of its being generated only on its rising into the colder air.

Again, Herschel says, "Vapor is subject to the same law of diffusion as other gases, and, while maintained in the vaporous state, to all the other laws of gaseous statics and dynamics." It is here worthy of special notice, that while this identity of vapor with all other gases is recognized by all writers, and thus acknowledged by Herschel himself, yet, when brought to the test of practice, it is invariably ignored. The properties of gases, for instance, in their relation to water, and when mixed with it, are accurately described by Dalton, showing distinctly how the water merely acts the part of a vacuum, the condition of their mixture being purely *mechanical*, and not *chemical*. Yet nowhere do we find this recognized in practice. It is impossible to describe this identity of vapor and other gases in more decided terms than in the above extract: yet, to suppose that vapor would give out its heat to water, would clearly imply a *chemical* and not a mere *mechanical* action.* It is also clear that Herschel had some misgivings on the subject when he says, "Vapor is subject to the same law of diffusion as other gases and while *maintained in the vaporous state*," etc. This appears to be an unmeaning distinction. When not in the vaporous state, it is clearly not vapor; no more than carbonic acid would be gas when

* "It appears to me (says Dalton) as completely demonstrated as any physical principle, that whenever any two or more gases or *vapors* are put together, into limited or unlimited space, they will finally be arranged, each as if it occupied the whole space." Again, "All gases that enter water and other liquids by means of pressure, and wholly disengaged again by the removal of that pressure, are *mechanically* mixed with the liquid, and not chemically with it. Gases, so *mixed with water*, retain their elasticity or repulsive power amongst their own particles just the same in water as out of it; the *intervening water* having no other influence, in this respect, than *a mere vacuum*."



condensed in the state of a liquid. On the whole we see how far we are from having correct or accurately defined ideas on the subject. The law of diffusion, while it expresses the main characteristic of *elastic*, in opposition to *non-elastic* fluids, demands a further inquiry as regards its practical application.



In reference to evaporation, and to illustrate this law, and that it may, as Dalton says, "be visibly depicted to the external senses," let the stars in *A* represent the vapor *in the water*, in an open vessel, at an indicated temperature of, say 200°. Let *B* represent the same after evaporation has reduced that temperature to 150°, and *C*, on a further reduction to 100°.

We here see, under the influence of diffusion, and by virtue of the mutually-repellant property which the atoms severally exercise, the vapor, so to speak, *filling the vessel;* the atoms, in the express words of Dalton, "enlarging the distances of the centres of their particles; or, which is the same thing—the diameters of the spheres of influence of each particle."

If, then, we suppose the upper line of stars to represent the *surface stratum* of vapor atoms in the water, and where they would be in contact with the air above them, these, like so many balloons, and by the mere effect of buoyancy, will rise, impeded alone by the pressure of the superincumbent atmosphere.

On the escape of this upper stratum of vapor atoms, and consequently, of the heat each respectively contained, with the continuing process of diffusion, the remainder being fewer in number, will have their respective distances still further enlarged, and still produce a homogeneous state, as to temperature, as shown in *B*. The same process will necessarily produce similar results, as shown in *C*.

We have here, then, a continuing harmony between the loss of vapor, and the loss of temperature, indicative of a corresponding harmony between the weight and heat of each evaporating atom. Thus, and confirmatory of theory, we are enabled, by observation and logical induction, to arrive at the most accurate results, establishing a positive law, namely, that as each atom of vapor has its equivalent unit of heat, an increase, or diminution in number of the one, must have its correlative in a commensurate increase or diminution of the other.

A further result is necessarily deduced from this harmony of *heat* and *quantity*—namely, that of *time*. As the escape of this surface stratum of vapor leaves a smaller gross quantity behind, so each successive surface stratum will contain a smaller number of atoms—the result being, that a commensurately longer time will be required in producing given amounts of evaporation, that is, given reductions in the gross weight of the body of water. Thus we see how the reduction of temperature must be the mere co-efficient of the loss of vapor.

Dalton made some accurate experiments on these points; his attention, however, was directed to the question of *time* rather than the corresponding differences in *temperature* and *weight*.* Numerous experiments have since been made, but which need not be further enlarged on, all establishing the fact, that the loss of *weight*, by evaporation, as it is the *cause*, so it must be commensurate with the loss of temperature—*time* alone being the varying incident in the process.

A further cause of increase in the *rate* of evaporation will be the more rapid removal of the rising vapor. On this Herschel observes:—"Evaporation is accelerated by wind blowing over the evaporating surface, which removes the *generated vapor, as fast as it is produced.*" Here again the main question is passed by, while the use of the terms *generated* and *produced* lead us to infer that this vapor had no existence until the moment of its escape from the water; that it was, in fact, generated or produced only on its rising into the air: a result which would be utterly inconsistent with the relation which heat bears to vapor, and inferring that vapor could be generated by the influence of the *colder atmosphere.* We have, however, given abundant proofs of the absolute existence and presence of vapor, rising from the bottom to the surface of a body of water from the very moment the heat is applied.

Now, the direct and only effect of a current of air passing across the surface of the water is, the reducing the

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* "Water, freely exposed to the air, evaporates at all temperatures, even when in the state of snow or ice. The rapidity of evaporation is, however, much increased by *warmth*. Thus, in a calm atmosphere, Dalton found that when, from a certain surface, the evaporation from boiling water proceeded at the rate of forty grains per minute, it was 20 grains at a temperature of 180°, 13 grains at 164°, 10 grains at 132°, and so on."—*Herschel's Encyclopædia Britannica.*

pressure or weight of the superincumbent air, thus giving greater facility for the rise and escape of the successive surface atoms of vapor. It seems impossible then to resist the inference, that the vapor could not have been produced or generated at the moment of its escape, but must have previously existed in the water, and so diffused as to produce the continuing homogeneous temperature throughout. Water, consequently, does not evaporate, but merely parts with its vapor. In fact, the water—the liquid mass—has no direct action or influence on the process of evaporation, and is merely the denser medium in which, at the time, it happens to be; and which, acting the part of a vacuum, gives scope and capability for its diffusion.

Herschel again observes: "Evaporation never takes place without the abstraction of heat from the evaporating surface." This is but the enunciation of a mere truism. It would be as if it were gravely stated that we cannot remove any portion of a body of water without an abstraction of a commensurate weight:—*heat* being as much an element of *vapor* as *weight* is of *water*. It would then have been more correct in terms and fact, to have said, that evaporation, being but the escape of the vapor, so it necessarily must be that of the heat, which is the very element of its existence as vapor.

Repeating what may be called the "vulgar error," he adds: "Evaporation is that process *by which liquids gradually assume the gaseous form*, or, by which an invisible vapor is continually detached from the surface of the water." Strange that it did not occur to him to ask where this "invisible vapor" came from, and how it was formed.

That liquids can assume the gaseous form by *evaporation* is in truth a grave error; and coming from so high an authority, may well be pleaded in justification by writers

of a lower standing. It is only necessary to say, that liquid atoms can alone assume the gaseous form when, and where, they receive those equivalents of heat by which they acquire that diffusive and mutually-repelling property which is their characteristic element.

From the numerous extracts here made, it is evident that due consideration has not been given to the important distinction between vaporization and evaporation; and that how and where this "invisible vapor" is actually formed, remain unsettled problems in chemistry. Why this should have so long continued to be so, can only be accounted for by our unwillingness to investigate for ourselves, and the habit of adopting the views of those who have gone before us, without question or inquiry, and regardless of their oversights and errors. We cannot close this section more appropriately than by the following quotation, so much as it is to the point, and so descriptive of our progress in reference to the subject under consideration: "If we trace the history of any science, we shall find it a record of *mistakes and misconceptions;* a narrative of misdirected and often fruitless efforts. Yet, if amidst all these, the science has made a progress, the struggle through which it has passed, far from evincing that the human mind is prone to error rather than to truth, furnishes a decisive proof to the contrary, and an illustration of the fact, that in the actual condition of humanity, *mistakes* are the *necessary instruments* by which truth is brought to light, or, at least, indispensable conditions of the process."

SECTION XII.

ON BOILER EXPLOSIONS.

NUMEROUS instances have occurred where explosions of boilers have taken place on the steam-engine being set to work after an interval of rest. This circumstance has given rise to many speculations, and it certainly appears unaccountable that the very act of liberating the steam, and thus, as it were, relieving the boiler of a portion of its pressure, should be followed by a practical augmentation of it. It is manifest, therefore, that there must have been some direct, though hitherto unexplained cause for such increased pressure, when a diminished one might reasonably have been expected. It is clear, also, that there must have been some rapid, and even sudden action, which is irreconcilable with a gradual generation of steam, and increase of heat.

In explanation of this anomalous augmentation of pressure, several writers have assumed what they term a sudden "*flashing* of the water into steam." This, however, is so wholly opposed to the effect which characterizes the gradual addition of heat, and the progressive change from the liquid to the vaporous state, as to be wholly inadmissible. It would imply some sudden action on the part of the water, *as a body or mass*, apart from its *elementary particles*, and without any additional accession of heat. Besides, the sudden *flashing* of water into steam, which means the *sudden conversion of liquid into vapor*, is opposed to all known ideas of the nature of matter in the

liquid state, and that change of condition from attraction to repulsion among its particles which would be the necessary consequence of their becoming vaporous.

Other writers are of opinion that when, on the opening of the valve, the steam is allowed to escape, an undue *agitation* of the water is produced, by which it is dashed against the hotter portion of the furnace plate, and more especially, if any part had been left uncovered by a neglect in the supply of feed water, assuming that a largely increased quantity of steam, with a corresponding increase of pressure, would be the result.

This theory is at once disposed of by the fact that explosions are equally attendant on an *overcharge*, as an *undercharge* to water, and that it attributes results on the largest scale to comparatively insignificant causes—a *maximum* of effect from a *minimum* of power.

A detail of the supposed causes of the sudden production of large quantities of steam, and consequent explosions, with numerous well-selected cases, was given a year or two ago in a series of papers in the London *Engineer*. It is unnecessary, therefore, here to do more than briefly enumerate the leading heads under which those causes may be arranged, viz., first, "electricity;" second, "decomposed steam;" third, "overpressure;" fourth, "explosions at ordinary pressure;" fifth, "the momentum of combined steam and water." This last is examined in reference to a statement made in a letter from Mr. D. K. Clark, and which is given *in extenso* below.*

* "*To the Editors of the Mechanics' Magazine.*

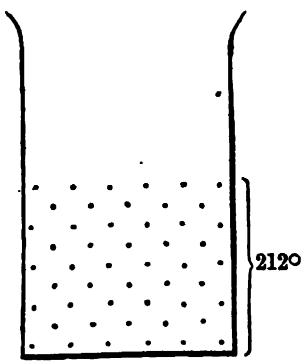
"11 Adam Street, Adelphi, London, February 9th, 1860.

"GENTLEMEN:—I have within the last few months given some attention to the subject of boiler explosions, their causes, and their *rationale*. I observe in the discussions that have appeared in contemporary papers, that *the percussive force of steam suddenly disengaged from heated water in a boiler*, acting

Let us now consider this subject practically. In section 6, on Ebullition, and 8, on the Presence of Steam in Water, it has been shown that so soon as the water has become saturated with steam, and the temperature of 212°, or thereabouts, has been reached, an uniformity, both of temperature and pressure, begins to prevail, and *continues*, both above and below the water level, indicating an uniformity *in quantity* of steam in both places. In other words, that each cubic inch of space in the boiler contains the same quantity of steam. The annexed figures will sufficiently illustrate this.

Let Fig. 40 be an open vessel half filled with water, having two thermometers suspended in it, the bulb of the one being *below* the water level, and that of the other above it. On heat being applied from beneath, steam will be generated and become diffused through the water until the point of saturation and the temperature

Fig. 40.



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against the material of the boiler, is adduced in explanation, and as the cause of the peculiar violence of the result of explosion. Now, a little calculation would show that the percussive force of steam is not capable of such destructive results as are occasionally produced; and I beg to suggest that the sudden dispersion and projection of *the water* in the boiler against the bounding surfaces of the boiler is the great cause of the violence of the results: the *dispersion* being caused by the momentary *generation of steam* throughout the mass of water, and its efforts to escape.

"It carries the water before it, and the combined momentum of the steam and the water carries them like shot through, and amongst, the bounding surfaces, and deforms, or shatters, them in a manner not to be accounted for by simple over-pressure, or by the simple momentum of steam.

"Your obedient servant,

"D. K. CLARK."

of 212° has been reached; the thermometer above the water, as presently will be shown, varying according to circumstances.

Let Fig. 41 represent the vessel covered and steam-tight, and the heat further applied. In this case the chamber above the water line will become equally filled with steam, and both thermometers will then stand at 212°, as there marked, the *dots* representing the steam atoms below, as the *stars* do those above the water line.

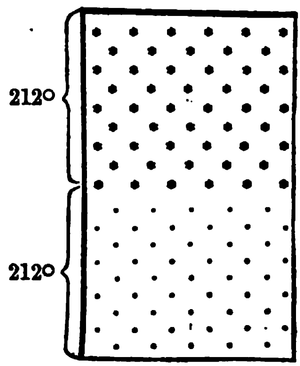


Fig. 41.

Let Fig. 42 represent the same vessel under a further application of heat, and when an accumulation of steam will continuously and uniformly take place, both in and above the water, as there shown by the increased and corresponding number of dots and stars in both places. In this case, then, we have a continuing uniformity of quantity, temperature, and pressure—say at 220°, or any higher degree, as the heat may be applied.

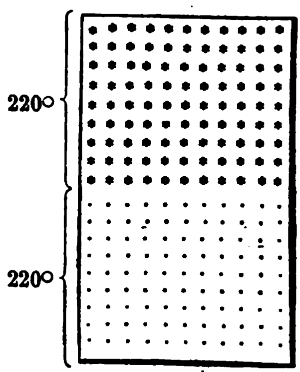


Fig. 42.

Let us now practically test these three states by the following experiments. In illustration of Fig. 40, heat was applied under an *open* vessel half-filled with water, and having two thermometers in it, the one in the water and

the other one inch above it. The following was the result (fractions of minutes or degrees of heat not being noted):

Temperature in the water.	Temperature above the water.	Time for every ten degrees of temperature.
70°	70°	
80°	75°	3 minutes.
90°	81°	2 "
100°	87°	2 "
110°	92°	1 "
120°	97°	1 "
130°	102°	1 "
140°	106°	1 "
150°	111°	1 "
160°	116°	1 "
170°	120°	1 "
180°	125°	2 "
190°	131°	1 "
200°	137°	2 "
210°	143°	2 "
212°	149°	

We here see that from the moment the heat was applied, there was an unremitting evaporation, or escape of steam, from the water into the air above it, and indicated by the thermometer, as noted in column 2.

On the temperature reaching 212°, the state of the water and steam in it may be taken as represented in Fig. 40, and as being at the point of saturation.

The next experiment in illustration of Fig. 41 was made in a steam-tight vessel—one of those described at page 182, Fig. 35—the vessel with two thermometers being half-filled with water. The following were the results:

Temperature in the water.	Temperature above the water.	Time in minutes.
70°	70°	
80°	75°	1 minute
90°	84°	1 "
100°	94°	1 "
110°	104°	1 "
120°	114°	1 "
130°	124°	1 "
140°	134°	1 "
150°	144°	1 "
160°	154°	1 "
170°	164°	1 "
180°	173°	1 "
190°	183°	1 "
200°	193°	1½ "
210°	204°	1½ "
212°	212°	

Here the steam rising out of the water into the close chamber *above it*, with the consequent increase of temperature, followed close on that of the steam *in the water*. This was what might have been expected; the steam, however, being generated in the water, and then passing into the space above it, must necessarily take the lead in quantity in the former. Nevertheless, its escape is so rapid, that it is throughout but a few degrees less in quantity than in the water itself.

This experiment illustrates what is represented in Fig. 41, the points below and the stars above being equally numerous in both places, and indicating an uniformity of temperature *when at the boiling point* under atmospheric pressure.

In both these experiments we have an unquestionable refutation of the prevailing theory of the steam being only

generated at the boiling point. Were that theory correct, the temperature in the space above the water line could not have been affected, but which was occasioned solely by the formation and escape of the steam, and which we see took place from the moment the heat was applied.

The third experiment in illustration of Fig. 42 was a mere continuation of the heat, with a further increase of steam and temperature—both thermometers indicating a continued uniformity of quantity, temperature, and pressure.

These experiments are so easily made, and withal so reliable, that it is a matter of surprise they have not been repeated or commented on by writers before they commit themselves to the unexplainable theories we find recorded. Now, the thermometer, being so trustworthy a test both of temperature and pressure, furnishes a sufficient refutation of those here enumerated. No boiler, then, should be used without having one inserted in it, so as to pass into the water, where it will become a more reliable test than the ordinary mercurial pressure gauge. We have everywhere tables of the comparative temperature, force, and pressure of steam; yet, strange to say, we find very little attention paid to them in practice.

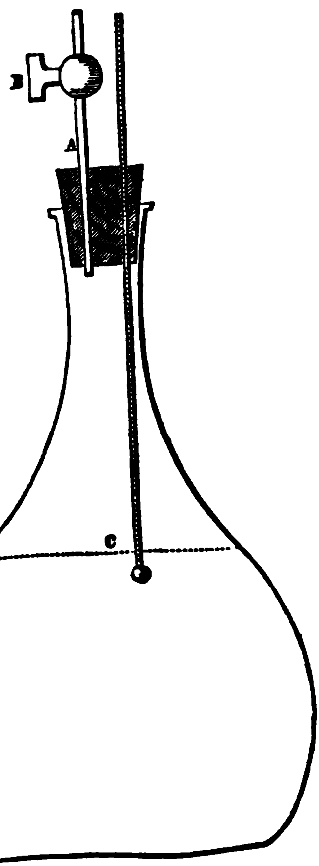
Let us now examine the subject with reference to the cause of the sudden increase of the pressure and risk of explosion.

We must bear in mind that, although under the influence of *diffusion*, there will be throughout a homogeneous condition as to the quantity of steam present, that portion which is below the water line will be in a medium—the water—denser than that above it. The water and the space above it being thus distinct and separate media, may here be considered as *separate chambers*, though still in

free connection. If, then, the steam in that dense medium —the water—be by any means liberated and passed into the lighter medium above it (these densities being as 830 to 1), there would necessarily be a *pro tanto* increase in quantity, and consequently in pressure, in the latter. This difference would also be increased in proportion as the area above the water line would be less than that below it, as thus illustrated.

Let Fig. 43 represent a five-pound bottle, with a cork fastened down by wire, through which the copper tube *A*, with the stop-cock *B*, was introduced, a thermometer being inserted through the cork, as here shown. On heat being applied from beneath, and the temperature raised to 214°, or above it, the stop-cock was opened. The result was an immediate discharge of the steam from the water medium into the space above it, accompanied by the appearance of violent ebullition, forcing much of the water before it, and a consequent discharge of both water and steam through the tube, with a rapid depression of the temperature to 212°. (If two

Fig. 43.



thermometers were introduced, the bulb of the one being *below* the water line, and that of the other *above* it, both will be found simultaneously to fall to 212°.)

The experiment was then repeated, the quantity of water being so increased as to make the bottle *nearly full.* On the temperature being again raised above the boiling point, say to 218°, and the stop-cock, as before, opened, the result was a still more violent discharge of steam and water from below; a considerabe portion of the water, together with the steam, being forced out, and rising to the ceiling, descended as from a shower bath.

We have here a miniature practical effect of what takes place in a boiler under similar circumstances, the violence of the action being increased in proportion as the area of the water space exceeds that above it.*

The necessary inference is that the risk of explosions is greatly increased by *the increase of water in the boiler,* every cubic foot of which, beyond what is absolutely necessary for the generation of steam, being an additional source of danger, when there is any sudden liberation of the steam which it contains into the reduced space above it.

Now, the determining the mere question as to the relative value of a *large* or *small* boiler, for an engine of any given power, on the score of efficiency or economy, is of itself worth the whole labor of the inquiry. Again, the determining, on sound, intelligible principles, the comparative value of the areas of space *below* and *above* the water line, is a matter of great practical importance. These are points, however, which receive no attention from those to

* This experiment should not be made with distilled or filtered water, but with water containing its ordinary quantity of motes or foreign matter, that ebullition may take place.

whom the construction and details of boilers are usually entrusted. If even we ascend higher, and look to those at the head of the first engineering establishments in the kingdom, we find the same neglect of those chemical principles on which the perfect combustion of the fuel absolutely depends. Look, for instance, to the plan of the boilers of the *Great Eastern*, designed by the late Mr. Brunel. It is only necessary to say that we there find, on the one hand, the greatest and most palpable violation of all natural and chemical laws, as regards effecting combustion and the generation of heat; and, on the other, the application of that heat in the generation of steam. The result is, not merely a large and wasteful expenditure of fuel, but a serious impediment to the generation of steam by the presence of soot, which lines the interior of the tubes.

In confirmation of the existence of steam *below* the water line, it may be asked how otherwise it could be explained that the temperature, both in the water and in the chamber above it, should be so instantly and simultaneously reduced? That the temperatureabove the water should be rapidly lowered to that of 212° is at once understood, as the result of the escape of the steam; but wholly unintelli gible, as regards the space *below the water surface*, except on the supposition that that space contained *steam* as well as *water*.

If the heat had been absorbed by the water, still in the liquid state, this sudden depression of the temperature could not possibly have taken place. It would be utterly opposed to all well known principles, as regards the transmission of heat between bodies, that either solids or liquids could be instantaneously deprived of their heats. The parting with the heat by one body, and its being

taken up by another, must be *progressive*, the one giving out heat only as the other can absorb it. As well might we expect that one ton weight of iron or lead, at a high temperature, could part with its heat *instantaneously* to another, as that a ton of water, supposed to be merely heated, could instantaneously part with its heat, and be reduced—say from 312° to 212°.

The rapidity with which the thermometer immersed in the water indicates a corresponding depression with that in the space above it, ought, therefore, to convince us that it was not the *water* as a liquid mass that contained the heat, or acted on the thermometer, but the *steam*, each atom in its escape from the water medium carrying away its own heat. In a word—that the cause of the indicated and simultaneous reduction of temperature was the same in both cases, namely, the escape of the steam which each contained.

Steam may almost instantaneously be discharged from the water, and the temperature as instantly reduced to 212°, the saturating point; whereas, *mere heat*, supposing it had been absorbed by the water, could not possibly be so disposed of. This single fact should be conclusive on the subject.

The violent movement which takes place in boilers where the engines have been set to work after a time of rest, though observed by so many, has never been satisfactorily accounted for. On this a writer (see the *Engineer* of Jan. 20, 1860, page 40) observes:—"One great cause of boiler explosions is, that when the engines are standing the water in the boiler is not in motion, when under pressure [this is contrary to fact, as may be tested in the progress of the last experiment, Fig. 43], and under such circumstances it may become heated far above the boiling

point; but as soon as the pressure is reduced in any way, as by starting the engines, the water is *violently agitated, producing a volume of steam* which cannot escape, and an explosion follows as a matter of course."

Here, the producing—that is, the generating—a volume of steam is indirectly attributed to the removal of pressure, and directly to the agitation. There is no question that the water is violently agitated; the cause of this agitation, however, remains still unexplained, and is here altogether mistaken, such being solely the effect of the steam suddenly escaping from the denser medium of the water, and consequently forcing, mechanically, the water before it, and out of the vessel. In this case the writer has overlooked the anomaly of a liquid being converted into steam without any further increase of heat.

Again, assuming that mere *agitation* had "produced the volume of steam," and mistaking the effect for the cause, he adds: "Were an agitator fitted in each boiler, to be kept in motion when the engines are standing, the cause of this explosion would be removed." Here the anomaly is still more apparent: for, if agitation caused the production of steam, it must be inferred that a continued agitation would render such production of steam continuous, thus wholly ignoring the action and effect of heat.

Looking, then, to the fact of the existence of steam in the body of the water, under the influence of the greater density of that medium, and its sudden liberation, the inference is, that the danger is increased when the boiler is *full of water*, rather than when there is a deficiency. Many proofs of this might here be given. In one case an explosion took place in a flue land-boiler, and a life was lost. At the inquest, engineers accounted for the explosion by assuming the water to have been allowed to

get too low. There was, however, abundant proof of the reverse. The over-pressure had caused the collapse and rupture of the flat flue at its largest and weakest point, immediately behind the bridge. The contents were forced, first against a mass of earthy matter behind the boiler, much of which it carried away with it, and then by a reverberatory action was driven to the front, where it forced down the yard wall, and then across a street thirty feet wide, covering with water and earthy matter the entire fronts of three houses opposite, each three stories high. Now, had the explosion been the result of an *imperfect supply* of water, none of these effects could possibly have taken place. As it was, the boiler must have been completely full, to have supplied such a quantity of water, and produce such effects.

Explosions rarely take place in marine boilers, mainly because the water space is comparatively so much smaller than that appropriated to the steam, as adopted in land boilers. Nothing, however, is more frequent than a deficiency of water, and consequent injury to the crown and side plates of the furnaces. In such cases, the plates becoming overheated and softened, necessarily yield to the pressure, and bulge inwards. If the plates be laminated, or otherwise unsound, they crack and leak. In one case the crown plates of the three furnaces of a boiler all bulged downwards to a considerable extent on the first trial trip, from a defect of the feed-water supply. The plates, however, being of good iron, the bulges formed regular curves; no leakings took place, and the boiler did good service for eight years. On being broken up, the bulged plates were found to be in no way deteriorated.

So much has been said of the required attention on the part of engineers and stokers against the danger of a

deficient supply of water, that they naturally fall into the opposite error, and, as if to make assurance doubly sure, take good care there shall be enough, letting the feed supply go on, and thus providing against the possibility of any deficiency. In this respect they see no source of danger from the glass gauges being full, and even without knowing the height at which the water may be in the boiler.

In the case of the explosion on board the Great Eastern, there can be no doubt of the chimney casing having been full of water while the heat was continued and the outlet stop-cock closed. Under such circumstances, and without regard to the temperature of the iron chimney, it became a mere question of hours, when the *accumulation of steam in the great body of water*, and *consequent increase of pressure*, would cause explosion.

It is also to be observed that the injury sustained by so many persons arose, not from contact with mere *steam*, but with *water*, charged, as every inch of it was, with such an excess of steam as almost to give it the character of liquid fire.

We have here, then, a practical illustration of the importance of this inquiry, and of a correct understanding as to the mode in which the heat is applied, namely, whether, as is generally supposed, it is absorbed by the water in its liquid form, producing what is termed *heated water*, or, in the state of steam, diffused through the water, and consequently exercising its properties of expansion and pressure.

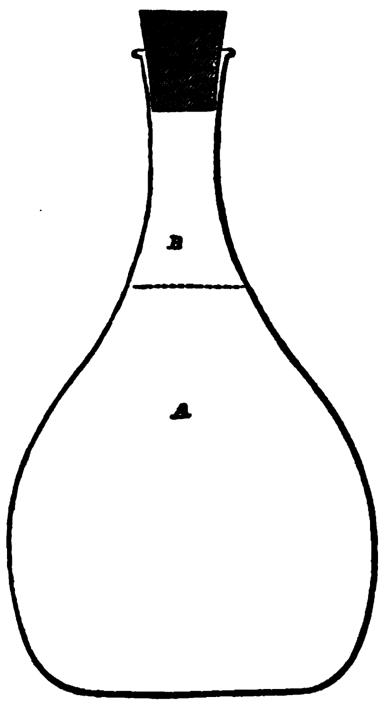
Keeping in mind that the steam *in the water* was in a medium 830 times denser than that above it, if this so compressed steam be suddenly liberated and added to that in the latter, at the moment when a boiler may be charged

up to its power of resistance, it will be manifest that it must give way, and in its weakest part, wherever that may be situated.

As already shown, the steam in the water, *in excess of saturation*, may be discharged into the space above it, whereas it cannot *reciprocate;* once out of the water, it cannot be returned, and the equilibrium of quantity, arising from diffusion, be again restored.

Let Fig. 44 represent a bottle containing, say 4 lbs. of water in *A*, charged with its saturating equivalent, or about eight ounces of steam, at the temperature of 212°, and with an excess beyond that equal to a pressure of 10 lbs. above that of the atmosphere. Let *B* represent the space above the water, and indicating a corresponding pressure of 10 lbs. If then, by any means, this excess in *A* be suddenly liberated and discharged into *B*, say, by a removal of the pressure, as when the engines are set to work, there will necessarily be a great and sudden increase in that space both in quantity and pressure.

Fig. 44.



Now there are several circumstances which will suffice to cause this escape of steam from the dense medium of

the water into the lighter one above it. Not only the removal of the pressure, but even the introduction of cold water, as from the feed pipe, is found to cause such a sudden grouping of the steam, as shown in the case of ebullition, as will effect its discharge from the water, as shown at page 116, Fig. 23.

Let us, then, suppose a boiler with a due supply of water, the thermometer indicating a temperature, both in the water and above it, say at 250°, with a pressure of two atmospheres, or 30 lbs. on each square inch. On the valves being opened and the engines set on, we can well conceive the rapidity and violence with which the steam would escape from the water into the space above it, and the consequences of such a sudden discharge, in the event of any part of the boiler being then at its maximum strain.

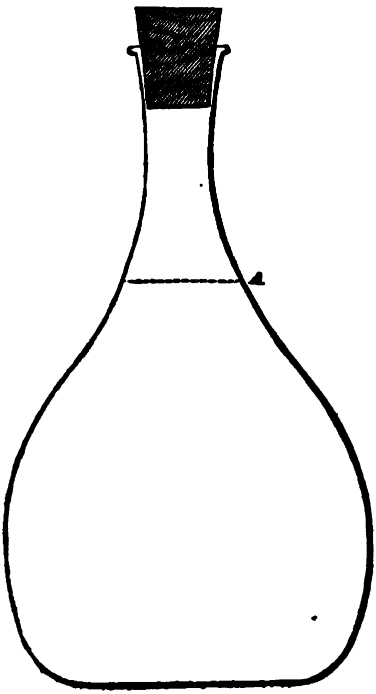
Under such circumstances no valve could possibly be sufficient for the escape of both the steam and water thus suddenly requiring relief; and from what we see in the small case of the experiment before us, we can appreciate the violence with which the steam would rush from the dense medium of the water when relieved from a pressure of perhaps 100 lbs. to the square inch. In this sudden enlargement and effort to escape we see, then, abundant sources of danger.

We know that the rapidity with which a balloon rises will be influenced by the density of the air medium at the earth's surface, and the lighter medium in the upper regions of the atmosphere. So must it be when the steam atoms, or, as we may call them, balloons, by any sudden diminution of the pressure, are enabled to rise from the water into the space above it. Those who may still be incredulous as to the existence of *steam in the water*, let

them make the following experiment, and answer the following queries.

Let Fig. 45 represent a bottle, nearly filled with water up to A. Let the cork be pressed in so as to bear a pressure equal to a temperature of 220°, or thereabouts. Let the heat be applied until the cork be forced out, which will be in a few minutes after boiling has commenced. On this *quasi* explosion taking place the entire mass of water will be seen violently agitated by the sudden liberation of its steam (the agitation beginning at the bottom), and its forcing before it a large portion of the water, which will descend like a shower bath. It will then be found that nearly one-half the water will have been forced out by the action of something. Let the operator then say what was it that drove the water out, and why the temperature was instantly reduced to 212°. He will be satisfied that nothing but the presence of steam, diffused through the water, could possibly have produced the explosion.

Fig. 45.



Some experiments were lately made in America by the Franklin Institute, in which explosions in iron and copper vessels were intentionally made. In one case a leak or fracture had existed, through which the steam escaped

with considerable force. It was then conjectured that the fracture had become suddenly enlarged, and which would be tantamount to a sudden opening of the engine valves and discharge of the steam. This idea, although entitled to consideration, leaves us still unenlightened as to *the cause* of the increased pressure, in the face of a reduced quantity of steam. It will be seen, however, that it bears directly on the subject here referred to, namely, the sudden liberation and consequent expansion of the steam from the water medium, at *the moment there was a maximum strain on the boiler.*

Dr. Ernst Alban, in his work on the high-pressure steam-engine (page 33), observes, in reference to the disproportion between the steam and water space: "The great quantities of steam and water tend to produce frightful consequences in case of explosion; the former by its great pressure and sudden expansion, the latter by its *instantaneous conversion into steam* by the removal of the pressure, as all the free caloric beyond the boiling point is spontaneously applied to the formation of vapor."

There are here several capital errors. 1st. The assuming a great pressure of steam *on the water*, whereas the pressure acts *upwards* equally as it does *downwards*—a fact which the uniformity of temperature in both places should have pointed out. 2d. The assuming an *instantaneous conversion* of the water into steam by the removal of the pressure, which, however, relieves the steam *in the water* equally and simultaneously with that in the space *above it.*

Again, there is really no ground for the supposed existence of free caloric—that is, caloric uncombined or unassociated with the water in the state of vapor or steam. To suppose an instantaneous conversion of the water into

steam would be to suppose the instantaneous conversion of attraction into repulsion, and that without any legitimate cause.

The Doctor concludes his work with the following exhortation: "*Prove all things; hold fast that which is good.*" We might here call on him to adopt his own recommendation, but to begin by *proving* what he merely *assumes* to be good.

SECTION XIII.

OF THE JET.

The jet may, at first sight, appear to be of minor importance, and scarcely entitled to recognition as an agent for developing power. All think they know its character, and the cause of its action and efficiency. On examination, however, it will be found to involve results, both practically and dynamically, of the deepest interest. To the jet belongs all that the increase of draft, and the consequent increase of combustion effect in the locomotive steam-engine. Although this is recognized by all engineers, yet none have examined the true cause of this *primum mobile* in reference to railway speed.

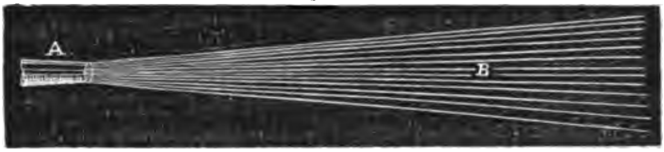
So important has been the result of the jet of waste steam in the chimney of the locomotive, that a contest of violence, and even acrimony, has long been carried on as to whom belongs the merit of its application at the time of the great rival contest on the opening of the Liverpool and Manchester Railway, in which Stephenson carried the day, his success being solely the result of the introduction of the jet of waste steam in the chimney. Of the peculiarity or cause of this result as a power, he was at the time wholly ignorant. Even to this day no one has yet given a correct scientific account of the true *modus operandi* by which the effect is realized. So little, indeed, was this known when first introduced into the *Rocket* engine, that the best mode of applying it, as regards area and position, was wholly undecided. All that was known, or considered requisite, was, that by throwing the waste steam from the

cylinders into the chimney, a great accession of draft was obtained, combustion accelerated, and a commensurate increase in the amount of steam generated. Subsequently, scientific men began to consider these points; the present position, however, of the jet at the base of the chimney, and the regulation of the sectional area of the jet, have been determined by *practice* alone, and without reference to principle.

From the use of the steam jet in the locomotive came its application in aid of the draft in marine boilers, where, of course, the benefit and effect of high chimneys, as adopted on land, cannot be available. Still, however, we shall see how little was known as to the principle on which the efficiency of the jet depended. In proof of this it may be mentioned that in a recent instance steam jets have been introduced into the chimneys of large vessels with so little judgment, that not only is their effect to a great extent neutralized, but it is accompanied with an unnecessary waste of steam. This will hereafter be explained and illustrated.

With the view of illustrating the principle on which the efficiency of the jet depends, let us take the most familiar instance, that produced by the common house bellows, used for urging combustion in our domestic grates. We all imagine that the effect is produced by the air which actually passes through the bellows and issues through the nozzle. That this is not the case, and is but a popular error, may be proved by allowing no air to enter the fire but what absolutely passes through the bellows and its nozzle. On doing so we shall find that but little effect would be produced in urging the fire. The following experiment will sufficiently illustrate this fact. Let *A*, Fig. 46, represent the nozzle of the bellows, and *B*

Fig. 46.



the conical stream of air impelled through it. Now, if that truly represented all the air that entered *the fire* we should find the effect to be altogether insignificant. To prove this, let the stream of air be absolutely confined to that quantity, by placing on the nozzle the tin conical tube *C*, Fig. 47, this tube being about six or eight inches long.

Fig. 47.

On urging the bellows we shall be surprised at the comparative insufficiency of the blast. Whence, then, it may be asked, is the additional supply obtained which makes the ordinary action of the blast so efficient? This may equally be made visible by placing on the nozzle, in place of the cone *C*, the cone *D*, Fig. 48, of the same size, but

Fig. 48.

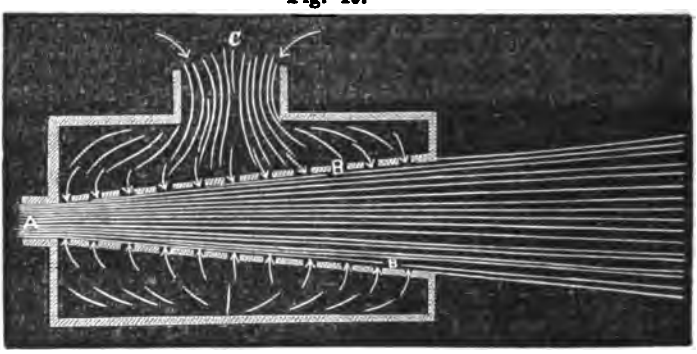
made of the perforated zinc plate used for the window blinds, or a tin cone perforated with numerous holes, each

of about one-eighth of an inch in width. If the bellows be then worked, strong currents of air will be found rushing into the cone through the numerous holes, and which will be found sufficient to restore the action to its ordinary force and effect in urging the fire. On holding the flame of a lighted taper to the holes in the perforated zinc cone Fig. 48, it will show the air forcibly drawn in through them, thus accounting for the increased quantity entering the fire. This additional supply may be called the induced current, and on this will be found to depend the great value and effect produced by the jet, whether it be of steam, air, or water.

The following will further illustrate the effect of the induced current.

Let Fig. 49 represent a tin cylindrical vessel about 5

Fig. 49.

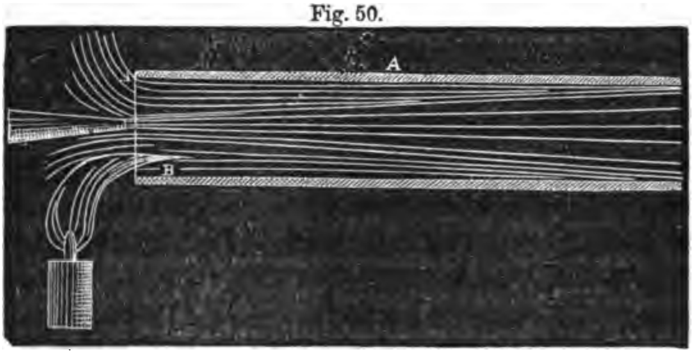


inches long and 3 inches diameter. *A* is the mouth-piece through which the air may be blown from the lungs. *B* is a cone of the perforated zinc plate, fixed in, and passing through the cylinder. *C* is an open space through which the air will enter to supply the induced current, to be drawn in through the perforations in the cone. On blowing smartly into the mouth-piece *A*, and holding the

lighted taper above the open *C*, the flame will be rapidly deflected, as pointed out by the arrows, thus showing the great additional volume of air which was entering, in aid of that impelled through the mouth-piece. If, however, the open space, *C*, be closed, the inefficiency of the blast will be strikingly evident.

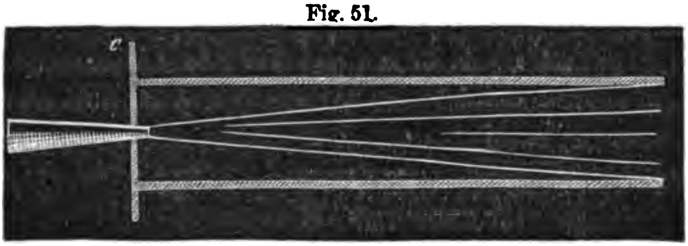
The following is a familiar mode of showing the great amount of the induced current, as compared with what is supplied by the direct jet. Let *A*, Fig. 50, represent a tin

Fig. 50.



tube of 4 feet long and 3 inches diameter. If the nozzle of the bellows be presented to the end of this tube, as shown at *B*, and the air be forced into it; on a lighted taper being held to its end, the direction of the flame will indicate the induced current entering the tube, as marked by the arrows. Now, to prove the inadequacy of the jet, when deprived of the induced currents, let the tin plate *C*, Fig. 51, be placed against the end of the tube, leaving a

Fig. 51.



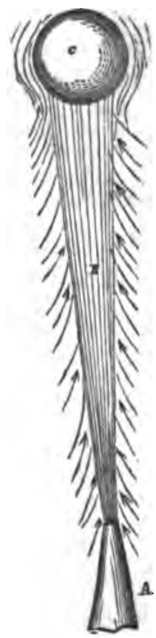
hole in the centre of the plate sufficient for the introduction of the nozzle of the bellows. The result will be remarkable, conveying the effect of an almost want of power in the action of the bellows. This will be the more apparent if we hold a taper at the further end of the tube, or a distance from it, to indicate the force of the blast.

The following are additional illustrations of the reality and importance of the induced current of air. We are all familiar with the boys' experiment of balancing a pea in the air by blowing through the stem of a tobacco pipe. Thus let *A*, Fig. 52, represent the pipe stem held verti-

Fig. 52

Fig. 53.

Fig. 54.



cally, and *C* the pea. On blowing through the pipe (the pea having been first balanced on its end), it will be forced

upwards, and sustained in the air several inches above it, as shown in the figure. In this case, no one doubts that the pea is not only projected into the air, but held in its position by the force of the jet and breath from the lungs; yet such is not the fact. The experiment will be more effective if made on a larger scale, thus: Let *A*, Figs. 53 and 54, represent the mouth-piece of a tin tube, having an orifice for the exit of the air of about one-eighth of an inch. If, then, holding the tube vertically, we place on its upper end the round cork ball *C*, of about $\frac{3}{4}$ or 1 inch diameter, the blast issuing from the lungs will project it 10 or 12 inches into the air, as shown in the figure.

Here, as in the case of the pea, it would be thought that the cork ball was sustained in its position by the sole force of the jet of air. So long as the jet is forced vertically upwards, there might be reasonable grounds for attributing the sustained position of the ball to the mere force of the jet. When, however, the ball is projected at a considerable angle as in Fig. 55, it is manifest there must be some other force in operation which prevents its falling over, as the ball could not be sustained at such an angle by the mere jet, since it must either be projected as a ball from a cannon, or it must fall downwards by virtue of its gravity. The induced current, however, illustrated in Figs. 54 and 55, then comes in aid of its support on whatever side it may be projected, sustaining its inclined position as shown by the arrows. This is further evident by observing, that as the intermittent current of the blast varies, the ball will return at a corresponding angle to the orifice from which it had been propelled.

Again, if the preventor tube *D*, Fig. 56, be placed on the mouth-piece, the effect of the jet will be so reduced as to be scarcely sufficient to move the ball.

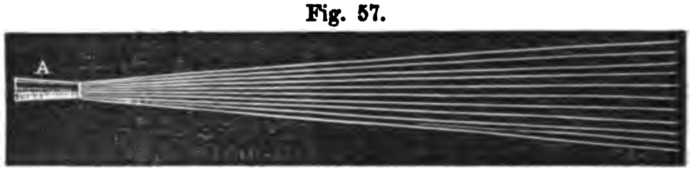
16

Fig. 55.

Fig. 56.

In further illustration of the value of the induced current, let the experiment be made with *steam* instead of *air*. Let Fig. 57 represent a jet of steam, say at 10 lbs. pres-

Fig. 57.

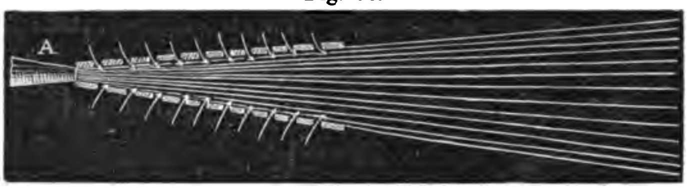


sure, issuing at *A* from a tube of an orifice of 1-10th of an inch. The steam will then be projected to a distance of 3 to 4 feet. Let Fig. 58 represent the same jet with the preventor tin cone *B* placed on it, as in former experiments by which the approach of any induced current will

Fig. 58.

be prevented. The result will be, that the force will be insignificant, the steam having apparently lost its force, as shown at *C*. If then, as before, we remove the preventor cone and replace it with one of the perforated zinc, the force of the let will appear to be restored as in Fig. 59,

Fig. 59.



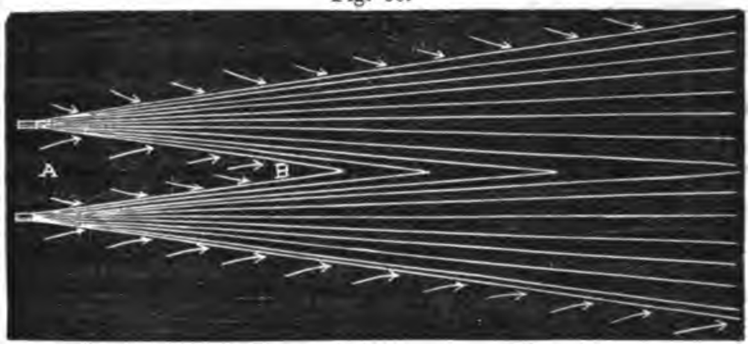
where, if the flame of a taper be held over the orifices, it will be drawn into them to supply the induced current, as shown when the jet was made with air.

The value of the induced current is fully exemplified in the jets of the ordinary Argand gas-burner, as also in the common blowpipe. In this latter case, if the pipe be used from the lungs, it is manifest it could have no value in promoting combustion, since, the carbonic acid and vapor issuing from it could not increase, or even sustain the combustion by which such powerful and intense heat is produced. We have, then, no alternative, but to attribute the generation of that heat to the pure air drawn in by the induced current.

Where jets are employed in aid of the natural draught of chimneys, whether they be of steam or air, there must ever be preserved a given relation between the sectional

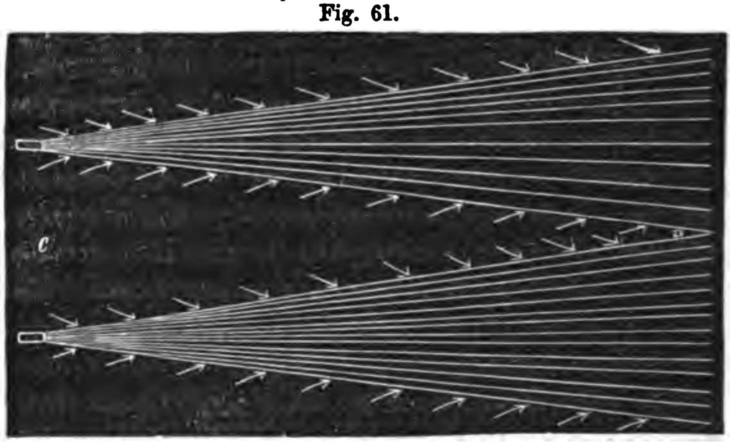
area of the jets, the distance at which they are apart, and the pressure or force by which they are produced. Thus, let *A*, Fig. 60, represent two jets of steam from two orifices

Fig. 60.



placed *one inch* apart. In such case they will meet, say at *B*. Here it is manifest they so far interfere with each other as, in a considerable degree to neutralize their respective induced currents. Let, however, the two jets be placed at a greater distance apart, as shown in Fig. 61; each will

Fig. 61.



then obtain a longer range for the action of the induced

currents. These facts are practically of great importance when jets are applied in aid of the chimney draft in marine boilers, where, from the disadvantage of short chimneys, the draft is rarely sufficient, and too often produced by an enormous waste of heat in producing a *hot* chimney, where a *high* one cannot be obtained.

Finding that in many cases steam-boilers were practically deficient in draught for the combustion of the *gas* in the furnace-chamber, although an adequate number of orifices had been furnished, it became a question why the plan adopted at Newcastle, for instance, and which was pronounced as "*practically perfect*," should fail in other cases. Many engineers asserted that they had tried the perforated air-distributors used at Newcastle, but had found them insufficient in effecting combustion and preventing the generation of smoke. None, however, had stated whether they had inquired if the due quantity, or indeed any quantity, had actually passed through the orifices so provided, an omission which at least indicated an unaccountable indifference as to the result. This was the more inexcusable when coupled with the fact, that the furnaces were so long, and the quantity of fuel used so great, that the throat, or passage over the bridge, was wholly inadequate, without taking into account the increased quantity of air that would otherwise have passed through the perforated plates, thus producing what is called a *back draught*, the flame actually going the wrong way, and issuing in strong jets at the doorway, where the fresh air should have been introduced. Under such circumstances it was manifest the perforations for the introduction of the air could be of no practical value, and hence the cause of the alleged failures. The principle, however, having succeeded in so many, or even a single instance,

should have been conclusive that it was not erroneous, but that the failure must have been the result of either an insufficient apparatus or imperfect manipulation. On inquiry it was found that, although the requisite number of orifices and adequate area had been provided, the actual quantity of air passing through them, when tested, was wholly inadequate; a fact that had not been suspected, the admission of the air being assumed to be the necessary result of the providing the orifices. This was the case in many of the vessels of the Dublin and other steam companies. The deficiency in the supply of air was then ascertained to be caused either by the absence of a sufficient natural draught, or the generation of so large a volume of gas in the furnaces, by reason of their great length and consequent great superficial area, as to be far in excess of what could be supplied by the perforated plates by the natural draught alone. The only alternatives then were, either to shorten the length of the grate area, or increase the draught by *mechanical means.* Trials were made several years back for effecting this latter object, as recommended by Peclet, in France, by the fan-blast. To this many practical objections presented themselves, as regarded marine steamers. The use of the steam jet and the induced current of air was then adopted, in many instances with decided success. By some, reliance was placed on the assumed value of the steam injected, under the erroneous impression that it became decomposed, and as such produced a new element of combustion. This was the case with Iveson, who, by his patent, injected a current of steam into the furnace-chamber above the fuel, and which plan was adopted by Iveson himself in the Bank of England printing department, under the late John Oldham; but was found practically inoperative. Had he known the

value of the induced current of the air, as the necessary accompaniment of the steam-jet, he would have placed the nozzle of the jet on the *outside* instead of the *inside* of the furnace. Some years back the use of the jets had been introduced into several steam vessels where the natural draught was insufficient, and which is almost generally the case in marine boilers, from the difficulty of bringing a sufficiency of cold air into the stoke-room, particularly where that is unduly heated. On this head engineers imagine the draught to be sufficient when they find the current of air by the *ash-pit* appears adequate to the urging the fire on the bars, altogether omitting, or forgetting, the large quantity required for the combustion of the gases in the chamber of the furnace, which they take no pains to supply.

In such cases, the mechanical use of the jet, either of steam or air, will be found to be an adequate remedy. The success, however, of the jet will exclusively depend on giving ample scope for the action of the induced current. In the application by Mr. D. K. Clark of the steam jets in the introduction of air for the combustion of the gas in the fire-box of coal-burning locomotives, the good effect must be derived exclusively from the induced current, seeing that the steam could not have any influence in promoting combustion.

It has already been observed that it is of the last importance to preserve a due relation between the sectional area of the jets and their distances respectively from each other. This has lately been brought to so practical a test as here to merit notice. The following is a forcible illustration of it:—

In a large steamer, lately built at Liverpool, a series of jets were introduced into the chimney in aid of the draught,

on the supposed principle which produces such efficiency in the locomotive. In the centre of the chimney, a short distance above the boiler, a 2½ inch tube, forming a circle of 2 feet diameter, was introduced. In this were drilled 60 orifices, each of ¼ inch area. Through these the steam issued continuously, in a vertical direction. Fig. 62 represents a plan of this circular tube, the jets being but one inch apart. That such an arrangement was made without a due consideration of the nature of the jet, or the principle on which its efficiency depends, will be manifest from the following.

Under the conviction that the several jets were so near each other that to a considerable extent they must have neutralized the action and effect of each respectively, an apparatus of the same dimensions was made and inserted in a tube representing the chimney. The air-meter, hereafter described, was attached so that the entire quantity of air brought in had to pass through it. As no other access of air was possible, the movements of the meter-vane gave a true measure of the quantity obtained in aid of the natural draught. The following experiments which were then made, will sufficiently explain the importance of considering the relative position of the several jets, and the value of this meter test.

In the first experiment, the 60 quarter-inch orifices, one inch apart, were all open, the pressure of steam in the boiler being 7 lbs., and which pressure was continued throughout, as shown in the table annexed. The steam having been let in, and a due time allowed for the whole to be thoroughly heated, the number of revolutions of the meter were accurately noted. In this case the revolutions were 540 per minute, and this may be taken as evidence of 540 cubic feet of air brought in aid of the draught.

Fig. 6.

Fig. 6.

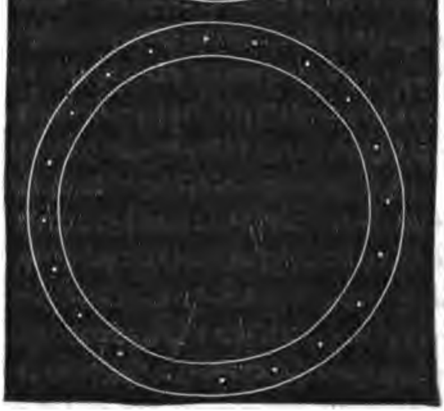


Fig. 64.

In the second experiment, each alternate orifice was plugged up, thus reducing the number of jets in action from 60 to 30, and increasing their distances apart from one to two inches—necessarily with a saving of one-half the expenditure of steam. The result was, an increase in the number of revolutions of the meter (or cubic feet of air, per minute) from 540 to 625, as noted in the table (page 251), being a gain of 17 per cent. in the effective chimney draft, with but one-half the expenditure of steam. Fig. 63 represents the reduced number of jets, and their increased distances apart.

In the third experiment, a further reduction in the number of orifices and jets was made, as shown in Fig. 64, their distances being then three inches apart. The result was a further increase of revolution and cubic feet of air passing, from 625 to 745, or a further gain of 19 per cent., with a commensurate saving of steam.

Thus, the increased effect produced solely by placing the jets further apart, say from one to three inches, and thus allowing the several induced currents of air a more lengthened scope for action, was, an increase of draft from 540 to 745 cubic feet per minute, or a gain of 35 per cent., with a saving of two-thirds the quantity of steam. It is needless to dwell on the value of this source of economy.

The next series of experiments with the air meter consisted in the reduction of the areas or size of the several orifices and jets of steam, from one-fourth of an inch to one-eighth, and then to one-tenth of an inch. In each case, as shown in the table, the results were remarkable. We there see that 30 jets of even one-tenth of an inch sectional area, if placed at three inches apart were more effective than 60 jets of a quarter of an inch when placed

within one inch of each other, as was the case in the apparatus as originally constructed.

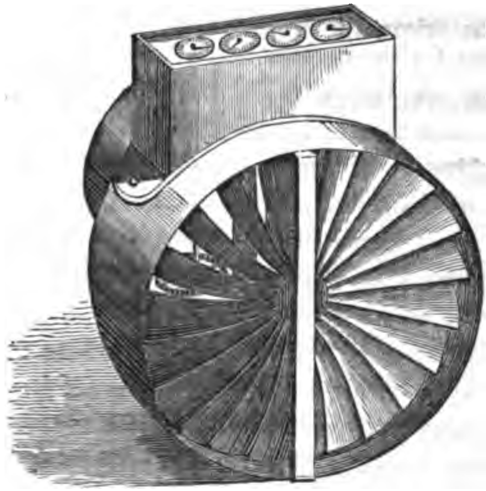
EXPERIMENTS WITH AIR METER AND STEAM JETS.

Revolutions of Meter per Minute.		Pressure in Boiler.	Number of Jets.	Size of Jets.	Gross area of Jets.
					Inches.
1-4th-inch Jets.	540	7lb	60	1-4th-inch.	2.945
	625	"	30	"	1.472
	745	"	20	"	.981
	725	"	15	"	.736
	700	"	12	"	.589
1-8th-inch Jets.	740	"	60	1-8th-inch.	.736
	615	"	30	"	.368
1-10th-inch Jets.	700	"	60	1-10th-inch.	.600
	600	"	30	"	.300

As regards the economy of steam, the inspection of the last column in the table, giving the gross area for the exit of the steam in each experiment, is sufficiently expressive. This needs no comment. Here we have practical results arising from a just appreciation of the principle on which the efficiency of the jet depends, in opposition to the mere random application of a most useful power in aid of the natural draft in chimneys.

By the use of the meter we come so near to the absolute quantity of air introduced that it practically satisfies all that may be required. The meter employed in the above experiments is shown at Fig. 65, being copied from a photograph of the actual machine. It will be seen that it consists of an ordinary circular vane, through which the air has to *pass* during the operation, producing motion like that of a windmill, increasing in rapidity in proportion to the strength of the current passing through it. This circular motion of the vane is transferred to a series of dials,

Fig. 65.



similar to those of the ordinary domestic gas meter. The number of revolutions of the vane are then truly recorded in units, tens, hundreds, etc., up to 100,000, thus enabling an experiment to be continued during above 100 minutes. The vane is eighteen inches in diameter, and having ascertained by a measured cylinder, after a due adjustment of the leaves, that the quantity passed during a given number of revolutions was equal to one cubic foot for each revolution, it was taken as a sufficiently reliable datum for calculating quantities. The number given by the index dials, then, will represent the number of cubic feet that have *entered* a furnace or been *drawn out*, as the case may be, or have escaped by the chimney, under the different circumstances of high or low temperature, with partial or complete combustion.

The vane is hung with such delicacy, and is so easily set in motion, that the loss by friction of the machine need scarcely be taken into account.

By the use of this apparatus numerous practical errors have been corrected touching the relative quantities of air required first for the combustion of the *coke portion* of the coal resting on the bars in the furnace, and passing through the ash-pit; and, secondly, the quantity required for the combustion of the *gas portion* in the chamber, the appearance of smoke giving unquestionable proof that the requisite amount of air had not been introduced.

Of the value of the jet system, and the important service it may be enabled to perform, it is important to consider the insufficiency of the natural draught in reference to the quantity of air required in large boiler furnaces, and in effecting perfect combustion. For this purpose it must be borne in mind, that the quantity of air absolutely required for the combustion of the gas portion alone of bituminous coal—as, for instance, the Newcastle—is at least one-half that required for the coke, or solid portion of the same. In a word, if the coke of a ton of coal requires 200,000 cubic feet of atmospheric air, the gas of the same ton weight will require 100,000 cubic feet—a fact but little thought of by boiler-makers, yet without this full equivalent of air much of the gas portion is not only wasted, but, by its conversion into smoke and soot, seriously impedes the action of the boiler, and in particular the tube portion of it. This latter, or 100,000 cubic feet, must be supplied to, and mixed with, the gas generated *above the fuel*, in the chamber of the furnace; while the 200,000 cubic feet have to be supplied by the *ash-pit*, and pass up through the solid fuel on the bars.

Now, where the heat-absorbing surface is well applied in the generation of steam (which is rarely the case) in marine boilers, the natural draught is invariably deficient. Here, then, we have, by the aid of an artificial draught,

through the medium of the jet system, the direct means of effecting the perfect combustion of the whole of the combustible portion of the coal. That this is imperfectly understood, or but little thought of, by those who construct such boilers, we have an absolute proof in the generation and evolution of that mischievous nuisance, the dense volume of smoke, which characterizes all steam marine boilers, using bituminous coal.

Finally, we may conclude that the efficiency of the jet system depends mainly, if not exclusively, on the amount of induced current, combined with a judicious arrangement of the several jets, keeping them in harmony with the area of exit of each, the pressure to which it is subject, and the space or freedom allowed for the several induced currents to exercise their maximum effect.

APPENDIX.

REMARKS ON THE MODE OF EFFECTING THE COMBUSTION OF COAL IN FURNACES.

THE following Testimonials, from high chemical authorities, have been selected from a number of others to the same import, and are here given in the order in which they were received and read by the author, in his Course of Lectures, at the Literary and Scientific Institution, Liverpool.

REMARKS BY MR. BRANDE.

Royal Mint, London, 26th November, 1840.

MY DEAR SIR: I am convinced that you are not only practically, but theoretically and scientifically, right in regard to the general views put forth in your Essay on the Combustion of Coal and the Prevention of Smoke. I have long advocated the principles which you have adopted, and have annually illustrated them in my lectures in the Royal Institution; but it unfortunately so happens that, when *scientific* men urge new views, or suggest the practical adoption of rational theories, they are neglected, be-

cause it is presumed they are merely founded on unsubstantial hypotheses; and, on the other hand, when *practical* men attempt to found improvement on scientific principles, they are sneered at as dabblers in science; and so, both become disheartened and disgusted, and the only people who are temporarily successful are *quacks*, who, pretending to what is called originality, and, neither referring to practice or science, steal from both; but, deficient in all knowledge of their own, ultimately mislead their followers, and, like the alchemists of old, not unfrequently deceive themselves.

Under these circumstances, it is really refreshing to find a person of your experience willing and able to blend practice with science and theory; and I have no hesitation in saying, that the views promulgated in your Essay are substantially founded upon just and scientific principles.

When the unburned hydrocarbons, which are produced during what is commonly called the *combustion*, but which, in reality, is principally the *destructive distillation* of coal in our ordinary steam-boiler furnaces, gradually mix with the air, they assume the form of what is called uninflammable smoke; but if they be examined *immediately fter* they leave the fuel and *before* they have blended with excess of cold air in the chimney shaft, they are found to be highly inflammable and rich in carbon. *At this point* it is that you judiciously admit the jets of air; and in so doing, every jet which enters the inflammable atmosphere within the flues becomes, as it were, the centre of combustion, and tends to increase the heat by burning and destroying that hydrocarbon which otherwise would go on to produce worse than useless smoke.

When air is admitted in a body to fuel, it never can effect those useful purposes which are obtained by admit-

ting it in due proportion to the intensely-heated inflammable vapors and gases; or, in other words, to the products of the distillation of coal at such temperature that they may take fire in its contact. In this way each jet of air which you admit becomes, as it were, the source or centre of a separate flame; and the effect is exactly that of so many jets of inflammable or coal gas ignited in the air; only, in your furnace, you invert this ordinary state of things, and use a jet of air thrown into an atmosphere of inflammable gas, thus making an experiment upon a large and practical, which I have often made on a small and theoretical, scale, in illustration of the inaccuracy of the common terms of "*combustible*" and "*supporter of combustion*," as ordinarily applied.

I fill a bladder with coal-gas, and attach it to a jet, by which I burn a flame of that gas in an atmosphere of, or a bell glass filled with, oxygen: of course, the gas burns brilliantly, and we call the gas the combustible, and the oxygen the supporter of combustion. If I now invert this common order of things, and fill the bladder with oxygen, and the bell glass with coal-gas, I find that the jet of *oxygen* may be inflamed in the atmosphere of *coal-gas* with exactly the same general phenomena as when the jet of *coal-gas* is inflamed in the atmosphere of *oxygen*. This is precisely your process. You admit a number of jets of air into a heated inflammable atmosphere, and so attain its combustion in such a way as to produce a great increase of heat, and, as a necessary consequence, destroy the smoke. You, in fact, convert what is commonly called smoke into fuel, at the *time when*, and the *place where*, this combustion can be most effectively brought about.

I have now given you my candid opinion respecting your views of the phenomena of combustion and their ap-

plication to the object proposed in your Essay. There are some of the minor points of which I cannot speak without further consideration; but these do not, in any way, affect the main object of the inquiry in its practical bearings.

I am always, my dear sir, sincerely yours,

WILLIAM THOMAS BRANDE

To Charles W. Williams, Esq.
Dublin Steam Company, Liverpool.

REMARKS BY DR. ANDREW URE, M.D., F.R.S.

London, 13 Charlotte Street, 28th Nov., 1840.

MY DEAR SIR: I have been perusing your treatise "On the Combustion of Coal and the Prevention of Smoke, Chemically and Practically considered," with extreme pleasure, and shall be able, in the course of a few days hence, to write you more particularly my opinion as to the great merits of your invention for obtaining the maximum calorific effect of fuel, and that without the nuisance of smoke. Meanwhile, I shall only state, that I consider your theoretical views to be chemically correct, and their application to steam-boilers to be fraught with the most signal advantages.

I remain, my dear sir, your very faithful servant,

ANDREW URE.

To Charles W. Williams, Esq.

REMARKS BY DR. BRETT.

Laboratory, Royal Institution, Liverpool, Dec. 3, 1840.

MY DEAR SIR: I have read your work on the Combustion of Coal, and have great pleasure in expressing my un-

qualified approval of the chemical views contained therein. I, moreover, entertain a strong belief that the most important practical results will flow out of their adoption, and that they will produce a new era in economical combustion. I shall take an early opportunity of sending you a more lengthened opinion as to the value of your discovery. In the meantime, allow me to subscribe myself, yours very respectfully,

R. H. BRETT, Ph.D., F.L.S.

To Charles W. Williams, Esq.

FURTHER REMARKS BY ANDREW URE, M.D., F.R.S.

Having now carefully perused your treatise "On the Combustion of Coals and the Prevention of Smoke, Chemically and practically considered," I cannot help congratulating you on the profound manner in which you have studied the phenomena of a furnace—phenomena which, like those of the freezing and boiling of water, had been for ages exhibited to the eyes of the philosopher and the engineer, without receiving from the one a scientific analysis, or leading the other to any radical improvement. You have fully demonstrated the defectiveness and fallacy of the ideas generally entertained concerning the operation of fuel in furnaces, and the errors consequently committed in their construction. *Nothing places in a clearer light the heedlessness of mankind to the most instructive lessons than their neglecting to perceive the difficulty of duly intermingling air with inflammable vapors for the purpose of their combustion*, as exhibited in the every-day occurrence of the flame of a tallow candle or common oil-lamp; for, though

this flame be in contact, externally, with a current of air created by itself, yet a large portion of the tallow and oil passes off unconsumed, with a great loss of the light and heat which they are capable of producing. Your quotations and remarks upon this subject must convince every unprejudiced mind of the justness of your views as to the imperfect combustion of the inflammable gases given out by coals on the furnace grate.

By experiments with Dr. Wollaston's Differential Barometer, made in several factories, where both high and low pressure steam was employed, I found that the aerial products of combustion from the boiler furnaces flew off with a velocity of fully 36 feet per second;* a rate so rapid as to preclude the possibility of the hydrogenated gases from the ignited coals becoming so duly blended with the atmospheric oxygen as to be burned. It is well known that elastic fluids of different densities, such as air and carburetted hydrogen, *intermingle very slowly;* but, *when the air becomes considerably carbonated, as it does in passing through the grate, and, consequently, heavier, it will not incorporate at all with the lighter combustible gases above it in the short interval of the aerial transit through the furnace and flues.* Thus there can be no more combustion amidst these gases and vapors than in *the axis of a tallow candle flame.*

Your *atomic representations are quite correct,* and will please all those who delight in tracing the working of nature into her formerly mysterious and inaccessible sanctuary.

You will remember, that when, about ten months ago,

* Experimental Inquiry into the Modes of Warming and Ventilating Apartments, in reference to the Health of their Inmates. By Andrew Ure, M.D., F.R.S. (Read before the Royal Society, June 16th, 1836.)

you laid before me the first draught of the specification of your patent furnace, with what delight I hailed your invention as the harbinger of a brighter day for steam navigation, where economy of fuel has become the *sine qua non* in regard to long voyages. I rejoice, that with the ample means placed at your command, you have since prosecuted the subject, through all its ambiguities, to a clear and conclusive demonstration of the efficacy of your plan for calling forth from pit-coal all its dormant fire, and diffusing it most efficaciously over the surfaces of boilers and along the flues. *I am more particularly pleased with your analysis of the combustion of the gases and vapors given out by hydrogenous coal*, commonly, though incorrectly, called bituminous, for it contains no ready formed bitumen, but merely its elements—carbon, hydrogen, and oxygen.

Having been much engaged during the two preceding years in experimental researches upon the calorific powers of different species of fuel,* I became aware that the hydrogenous constituents of coal underwent a most imperfect combustion, and found *I had been misled* for some time to the false conclusion that *the caking Newcastle coals afforded less heat than the non-hydrogenous anthracite of Wales. When I improved my method of burning the gaseous products first disengaged from coals, I obtained a greater quantity of heat from the so-called bituminous species;* a result quite in accordance with long-established chemical data. The immortal Lavoisier and Laplace ascertained that one pound of hydrogen, when burned in their celebrated calorimeter, melted 295·6 lbs. of ice, while one pound of charcoal melted only 96·5 lbs.—quantities very nearly in

* An account of these experiments was laid before the meeting of the British Association at Birmingham, and printed in the *Athenæum*, of Sept. 14, 1839.

the ratio of 3 to 1. Despretz gives the same of charcoal. It deserves to be remarked, that this ratio is exactly the inverse of that in which hydrogen and carbon unite with oxygen; for 1 part of hydrogen, by weight, combines with 8 of oxygen to form water; and 3 parts of carbon combined with 8 of oxygen to form carbonic acid gas, which is the product of the complete combustion of charcoal. From these and similar researches, chemists have been led to conclude that the heat afforded by different bodies in the act of their combustion is in the ratio of 315 to 104; thus proving beyond a doubt, *that hydrogen can disengage, in its combustion, three times more heat than the same weight proportional to the quantity of oxygen which they consume;* a conclusion which accords, also, with the principle, that the intensity of heat is proportional to the intensity of chemical action, as measured by the proportion of oxygen which enters into combination.

For the first accurate analysis of pit-coals we are indebted to Mr. Thomas Richardson, of Newcastle,* who published, a few years ago, in the eleventh volume of Erdmann's Journal fur Chemie, the results of an excellent series of researches on coals made in Professor Liebig's laboratory. He used the fused chromate of lead to oxygenate the carbon and hydrogen of the coals, with Liebig's new apparatus; and his results deserve entire confidence. In the earlier analyses of coals made by Dr. Thomson, myself and others, the peroxide of copper, which was employed to oxygenate the combustible matter, always left some of the carbon unconsumed, and thus occasioned unavoidable errors.

* An account of these experiments has been since presented, by Mr. Richardson, to the Natural History Society of Newcastle-upon-Tyne, and is printed in the Transactions, vol. ii. p. 401, and in the London and Edinburgh Philos. Magazine, vol. xiii., p. 121, for August, 1838.

1. Rich caking coal from Garesfield, near Newcastle, of sp. grav. 1·280, was found to contain as follows:

Carbon	87·952
Hydrogen	5·239
Azote and oxygen	5·416
Ashes	1·393
	100·

2. Caking coal, of excellent quality, from South Hetton, in the county of Durham, of sp. grav. 1·274, afforded,

Carbon	83·274
Hydrogen	5·171
Azote and oxygen	9·036
Ashes	2.519
	100·

3. The parrot coal of Edinburgh afforded,

Carbon	67·597
Hydrogen	5·405
Azote and Oxygen	12·432
Ashes	14·566
	100.

100 parts of these several kinds of coal take for perfect combustion (subtracting the oxygen contained in the coal) as follows:

1st.	266·7 parts of oxygen: giving out heat as the number	122·56
2d.	250·2 " " "	114·98
3d.	217·6 " " "	100·00

The quantity of heat is here presumed to be proportional to the quantity of oxygen consumed. M. Regnault published, in Erdmann's Journal, vol. xiii., p. 69, the following statement of his analysis of coals, which is regarded by Professor Lowig as very correct:*

* Chemie der Organischen Verbindungen, vol. ii. p. 88.

Newcastle coal, of sp. grav. 1·280, affording a much-inflated coke (quite akin to the Garesfield coal, if not the same), was found to consist of carbon, 87·95; hydrogen, 5·24; azote and oxygen, 5·41.

Every coal which contains much hydrogen, and, therefore, loses much weight by ignition in retorts, necessarily produces much smoke, with a great waste of heat in our common steam-boiler furnaces, for reasons which you have so well developed in your treatise. "When a carburetted hydrogen," says Liebig, "is kindled, and *just as much oxygen admitted to it as will consume its hydrogen, the carbon does not burn at all, but is deposited* (or separated) *in the form of soot;* if the quantity of oxygen is not sufficient to burn even all the hydrogen, carburets of hydrogen are produced poorer in hydrogen than the original carburetted hydrogen."* The above gas and smithy coals which, from their richness in hydrogen, are capable of affording the greatest proportion of heat by thorough combustion, afford often a much smaller quantity than the Llangennock, because the carburetted hydrogen which they so abundantly evolve is not supplied with a *due* quantity of oxygen, and hence much of their carbon goes off in smoke, and their sub-carburetted hydrogen gas in an invisible form. These results are quite accordant with my experiments on these coals with my calorimeter. At first, from certain defects in the apparatus, whereby the coals were imperfectly burned and a good deal of smoke was disengaged, I found that the best coals imported into London, such as Lambton's, Wallsend, Hetton ditto, and Pole's Main, afforded a smaller proportion of heat than the Llangennock, or even anthracite; but, *when I diminished these defects, I obtained much more heat from the Tanfield Moor coal than from the Llangennock, and more from this than from the anthracite.*

* Traitè de Chimie Organique, Introduction, p. 32.

In fact, *a coal which, like the Newcastle caking coal, contains* 5·239 *of hydrogen, is capable of giving out in complete combustion as much heat as if it contained an extra* 10½ *per cent. of carbon;* but instead of this additional heat, it affords in common furnaces much less heat than the Llangennock, though this is much poorer in the most calorific constituent, viz., the hydrogen.

The first operation which coals undergo, on being heaved into a common furnace, is distillation, attended with a great absorption of heat, and may be compared to the distillation of sulphur in the process of refining it, for which purpose much external heat is required. But, if the fumes of sulphur or the coals be, after ascension, intermingled with the due quantity of atmospherical oxygen, they will, on the contrary, generate internally, from the beginning, their respective calorific effects.

In the case of great steam-boiler furnaces, for which your patent is specially intended, since these are fed at short intervals, *your plan of distributing atmospheric air, in a regulated quantity, by numerous jets, through the body of the gasiform matter, is peculiarly happy, and must enable you to extract the whole heat which the combustible is capable of affording.* The method, also, which you have contrived for distributing the air under the surface of the grate will ensure due combustion of the coked coals lying there, without admitting a refrigerating blast to the fire. And, finally, your mode of supplying atmospherical oxygen will prevent the possibility of the carbon of the coals escaping in the state of carbonic oxide gas, whereby at present much heat is lost in our great furnaces,

ANDREW URE.

13 Charlotte street, Bedford square, London.
December 26, 1840.

FURTHER REMARKS BY DR. BRETT.

Laboratory, Royal Institution, Liverpool, Jan. 6, 1841.

I now take the opportunity of expressing more fully my opinion of your views concerning the combustion of coal and prevention of smoke, and it gives me the greater satisfaction so to do, in consequence of having enjoyed, on more than one occasion, an opportunity of examining, in your experimental furnace, when in operation, the different stages of coal combustion. Your furnace being so constructed that it allows of a close inspection of the changes which take place within the flue behind the bridge, no person, however imperfectly acquainted with those scientific principles upon which combustion ought to be effected, can avoid recognizing the great difference (a difference sufficiently palpable to the sight) which exists between combustion, as conducted in furnaces upon the old plan, and that which takes place in yours, when the inflammable matters receive *good air properly supplied.*

The importance of this supply of good, instead of vitiated, air, *at a proper time and place*, and the no less important influence which a thorough *commingling* of the substances to be burnt and the substance burning exerts in facilitating full and complete chemical union, has been satisfactorily shown in your treatise; and the injurious effects from a neglect of these precautions no less clearly demonstrated; and this appears to be quite manifest if we look to the nature of the elementary substances which enter into the composition of all ordinary pit-coal.

If coal be submitted to heat in large vessels, as in the coal-gas retorts, combustion does not take place, but the

elements pre-existing in the coal undergo new arrangements, giving birth to volatile substances, which pass off, leaving behind a solid carbonaceous matter or coke. These gaseous or volatile substances are sufficiently numerous, consisting of light and heavy hydro-carbons, napthaline, sulphuretted hydrogen, carbonic oxide, ammonia, cyanogen, free hydrogen, and sulphuret of carbon; all these compounds being produced from the five already-mentioned elementary bodies. A portion of the heavy hydro-carbon, or olefiant gas, suffers decomposition at the high temperatures of the gas retorts, carbon being deposited in no inconsiderable quantities even upon the roof of the retorts, light carburetted hydrogen passing off as gas.

This, then, appears to be the state of things which obtains when coal is submitted to destructive distillation without aerial contact. When, however, coal is submitted to the agency of heat for special calorific purposes *with* aerial contact, then actual combustion ensues, either partial or complete, according to the *manner* in which atmospheric air is supplied; the great object always being to prevent the formation, or, at all events, the permanent existence of those compounds, especially the nitrogenous ones already enumerated; instead of which, the only products ought to be carbonic acid and water, with some nitrogen, and, occasionally, a small quantity of sulphurous acid. The old mode of combustion in furnaces is manifestly incompetent to effect this perfect combustion, and only to be attained, in my opinion, by a plan based upon such principles as you have advocated.

Every one who observes the volumes of black smoke escaping from the chimneys of manufactories must be struck with the positive loss of fuel thus sustained; yet, not only is the black smoke lost for calorific effect, but

a further loss may be traced to the passing off of what may be called a smoke, though not visible—I mean unburnt carburetted hydrogen and carbonic oxide. You have been fully alive to the truth of this, and, by a felicitous contrivance, have shown that by mingling atmospheric air with inflammable gas, before they can escape unburnt, black smoke may be got rid of, or, in other words, that loss of fuel and consequent loss of heat may be avoided.

On one occasion, whilst watching the process of combustion during its different stages in your experimental furnace, at that period when the hydro-carbons had just undergone combustion, and the carbonaceous matter or coke was remaining on the fire-bars highly ignited, I observed, when looking into the flue, the phenomenon of a peach-colored flame mingled with one of a striking blue color. The former I believe to have been produced by the burning of cyanogen; the latter was undoubtedly due to carbonic oxide. To the interesting fact of the combustion of the latter gas, after that of the hydro-carbons, you will remember to have directed my attention on a former occasion. I need hardly say that, without a proper supply of atmospheric air, these gases would have escaped by the chimney, and so have been lost for calorific purposes.

I will conclude these observations by a remark or two on a point much insisted upon in your treatise, as it appears to me of great importance, namely, the *diffusion* of the atmospheric air and inflammable gases as much as possible into each other *prior* to combustion. In doing this you but follow out the rule which science and experience alike dictate, which is to cause as intimate a mixture as possible of those substances between which you desire that chemical union, and the previous mechanical arrangement of the bodies to be united should be, and are in your plan, carefully observed.

By causing atmospheric air to be driven *by jets* among the inflammable gases, you employ, as it appears to me, the only means practicable in operations on a large scale, of causing a sufficient mechanical admixture between the air and the gases to be burnt. By such means, too, you considerably extend the surface of any given bulk of atmospheric air admitted in the same way, as the surface of any given volume of water is greatly increased by causing it to pass in thin streams through a vessel containing numerous apertures.

The importance of a due supply of atmospheric air to coal-gas, and a proper admixture of the one among the other prior to combustion, may be shown by taking a piece of wire gauze, placing it nearly in contact with the perforations of a common gas argand-burner, and then turning on the gas and inflaming it. After it has passed the wire gauze, the flame will be found to be luminous and fuliginous, and a piece of platinum wire placed across it will be only heated to dull redness about the external part of the flame; but if, whilst things are in this condition, the wire gauze be raised so as to leave a space of some few inches between the orifices of the burner and the under surface of the gauze, the flame speedily loses its luminous properties to a great extent, is no longer fuliginous, and has so increased its calorific properties as to cause the platinum wire to be heated to a bright redness.

In the first experiment imperfect combustion takes place from an inadequate supply of atmospheric air and want of diffusion of the air and coal-gas among each other. Hence the luminosity of the flame, containing, as it does, solid unburnt carbon, and hence, also, its less powerful heating properties and fuliginous character. In the latter experiment, the air becomes mingled with the coal-gas before

passing through the wire gauze; and, when this mixture is inflamed *above* the gauze, perfect combustion takes place, no smoke is produced, and a great increase of heat obtained.

On the whole, then, I would observe, that your treatise is correct as to its theoretical and scientific principles; and, by your illustrations on the large scale of the furnace, you have demonstrated how science and practice may be made to harmonize, and have shown the importance of attending strictly to the *chemical* conditions on which combustion takes place, without which all must be error and uncertainty.

R. H. BRETT, Ph.D., F.L.S.

To C. W. Williams, Esq.

REMARKS BY DR. ROBERT KANE.

Royal Dublin Society, January 16th, 1841.

DEAR SIR: I received the copy of your work and the other papers illustrative of your views of the nature of combustion and of the construction of furnaces, and I consider your suggestions as being of peculiar value inasmuch as they indicate to the engineer and mechanist the principles on which alone the perfect combustion of fuel can be secured. They serve, also, as an additional proof, were such wanted, that complete success in art—the greatest economy in materials—and the most perfect utilization of its products can only occur when the *scientific* conditions of the process are clearly understood and made the *foundations of practice.*

In your furnace alone, of all the plans I have had an opportunity of examining, the conditions for *the complete combustion of all the constituents of the fuel are secured in the proper circumstances of quantity, time, and place.*

The introduction of air at the bridge and along the flame-bed, to supply the quantity of oxygen necessary for the combustion of the volatile products of the coal;—the diffusion of this air, secured by its issuing from a great number of small jets, and the consequent full combustion of the gaseous fuel before it leaves the surface of contact with the boiler, are elements of real economy and success in practice. The value of this, although, perhaps, obscurely felt by others, from the imperfection of the older methods, has been certainly first placed in its important and just aspect by your illustrations.

The formation of carbonic oxide in furnaces, to WHICH I FIND, IN YOUR WORK, FOR THE FIRST TIME, THE ATTENTION OF PRACTICAL MEN DIRECTED, has hitherto been a source of loss of fuel to a very considerable extent. When carbonic *acid* streams over a surface of ignited charcoal, or coke, it cannot be considered to evolve heat in taking up an additional equivalent of carbon. This is verified by the fact that the carbonic oxide thus formed gives out, in burning, only the same quantity of heat which the second equivalent of carbon should have given out if it had formed *carbonic acid directly.*

This heat you encourage, and hence, even at those periods when carbonic oxide is produced, you lose no fuel; whereas, in all the ordinary plans of avoiding *visible smoke*, the fuel is evolved in carbonic oxide, and is either silently escaping as *invisible* gas or burns at the orifice of the chimney, wasting there the carbon which should have been economized below.

I like the plan of your diagram very much. In popular use, and especially for the instruction of those who have not made abstract calculations their study, they are exceedingly valuable: so much so, that our illustrious and

venerable friend, Dr. Dalton, the Nestor of the physical sciences, as he has been termed by Dumas, proposed a plan not unlike yours, but which, from the very fact of its being more adapted to class illustrations than to the calculations by symbols (now so necessary in scientific chemistry), was kept very much out of view lately by the employment of the Berzelian notation. I am glad, therefore, that you propose *coloring these diagrams in a second edition*, for, when one writes, as you do, not merely for those who know a good deal, but for those who are not used to the kinds of ideas you wish to communicate, every adjunct which tends to awaken the senses to the subject is of the highest importance.

In conclusion, I beg to express my conviction, that, by giving to the scientific world the true philosophy of combustion upon the large scale of the furnace, and indicating to the mechanist the conditions upon which the proper construction of furnaces must rest, you have illustrated remarkably the value of science applied to the useful arts, and have effected considerable service to the public.

Believe me, my dear Sir,
Sincerely yours,
ROBERT KANE.

To C. W. Williams, Esq.

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